The Progress and the Spirit of Medical Science.

AN

ANNIVERSARY DISCOURSE,

DELIVERED BEFORE THE

NEW YORK ACADEMY OF MEDICINE,

NOVEMBER, 25, 1858.

BY

E. R. PEASLEE, A.M., M.D.

PROFESSOR OF PHYSIOLOGY AND PATHOLOGY IN THE NEW YORK MEDICAL COLLEGE.

[PUBLISHED BY ORDER OF THE ACADEMY.]

NEW YORK:

PRINTED FOR THE ACADEMY BY W. H. TINSON,

43 & 45 CENTRE STREET.

1859.

ANNIVERSARY DISCOURSE.

Mr. President and Fellows of the New York Academy of Medicine:

Standing in the presence of so many of my profession whom I have been accustomed to reverence, I might feel some embarrassment, did I not remember that I am placed here to-night by you and in your service; and that this is an occasion not intended especially to call forth your criticism, but to enkindle anew and to expand our professional sympathy. I would gladly, therefore, render the exercises of the present hour an entertainment to you, instead of a task. But I must select no trivial theme; and since the regular exercises of this association are mostly of a kind in accordance with the precept that—

> "That which before us lies in daily life
> Is the prime wisdom"—

I do not feel at liberty to select a strictly practical subject. I will, therefore, present some thoughts upon *the progress and the spirit of medical science, and its claims upon medical men.*

"Hitherto," says a French critic,* "medicine—

* Louis Peisse: La Médecine et les Médecins, Paris, 1857.

both science and profession—has had only detractors and apologists, believers or skeptics. It has been attacked and defended by special pleadings only; there have been arguments on both sides, but no decision. But where is the Kant who can and will tell us the fact? Shall we seek him in a professor's chair, or in an Academy? Wherever he is, he should show himself, for his time is come." It is with some faint hope of calling out the individual so much needed, that I have selected my present subject. The limited time allowed me on this occasion will of course compel me to treat some of its subdivisions in a very cursory manner; and entirely to omit others. But in what I may say, I will hope to avoid the errors both of the panegyrist of his own times, and of the "laudator temporis acti." I shall necessarily adduce many facts familiar to all medical men; though not, I will hope, for a trite purpose.

The medical profession, like the others, is somewhat inclined to cherish the idea of modern perfection. Like the other professions also, its members generally adopt the principles and acknowledge the influence of their predecessors without questioning their correctness. A candid inquiry into the real progress, and present condition of our science will, therefore, perhaps somewhat abate our complacency; but it should stimulate our highest efforts to advance it towards that perfection which all invoke, and would witness for themselves. We are, however, during most of our lives, too much immersed in strictly professional labors, to give much attention to these higher subjects; and are too prone to feel that

they do not particularly concern ourselves. But there should be times, when our *personal* responsibilities in these respects are presented to us; and I have assumed that this is appropriately an occasion of that kind.

There are those who assume that the human mind itself has undergone a progressive increase of power and perfection in modern times; so that we now seize, intuitively as it were, on what cost the ancients much study and application; and hence, know all they did almost as a matter of course. This assumption is, however, opposed to all observation, and to fact. The infant of 1858 requires as long a time to learn to talk and to walk, or to acquire any other accomplishment, for aught we can conceive to the contrary, as did the first-born of Cain. And there are no intellects now-a-days superior to the most brilliant of ancient times. The advantage of the moderns consists in the improved facilities for acquiring knowledge; and especially from the art of printing. A few weeks may now suffice to acquire an amount of knowledge it cost the ancients, perhaps, a thousand years to establish. But the *power* of acquisition is no greater now, and its necessity no less, than it was then. Nor is it by any means certain, or even probable, that the ancients knew so much less in the aggregate than the moderns, as is so generally assumed. They knew not precisely *what* the moderns know; but the world has not improbably forgotten quite as *much* as it knows. We have derived from the ancients, but a small part even of their recorded knowledge. But that little is calculated to give a

most exalted idea of the intellect and the learning of antiquity, since it has impressed its character upon all ages up to the present, and will impress all succeeding ages to the end of time. And some of the recently invented instruments of our art, and which were supposed to indicate a degree of progress and perfection never dreamed of by the ancients, are now known to have been used at least 1,800 years ago.* We, therefore, as truly as the ancients, can by labor alone improve, or even acquire, our science.

PART I.

THE CONSTITUENTS AND THE METHODS OF SCIENCE.

The following inquiries present themselves as preliminary to my main subject.

I. *What is the distinction between science and art?*

II. *What are the elements constituting an actual science?*

III. *What is the true character of medical science as compared with the other sciences?*

I. It is often asserted that science is the basis of all art. But it will be found in the majority of actual instances, that art is the forerunner, and not the fol-

* Especially the speculum vaginæ and speculum ani. See drawings of these and other instruments found in the ruins of Pompeii, in *Amer. Med. Monthly* for August 1858.

lower—the parent, and not the offspring—of science. It is the province of science to know; of art, to do. Science inquires and ascertains what is; art changes what is to something different. Science foresees and predicts the phenomena around us; art forestalls, controls, or prevents them; science discovers; art invents. The immediate object of science is truth; of art, either pleasure or utility. But we must try, before we can ascertain and know; the tentative must precede the positive: art in its primitive forms, must precede science. It is true that every effort of art is made in accordance with certain facts and principles; but such efforts may be repeated for centuries, before the facts or principles are discovered. Meantime the art is improving, though the science still remains unformed. Some of the inventions of the present day even, have not yet found a scientific basis. Hence in the early ages of society there are many arts, but no sciences.

The art of medicine, therefore, dates back to the earliest times; while our science is of comparatively modern date. The old adage—"Nusquam medicina non est"—refers to the art or practice of medicine alone; and implies that there never has been a time, in any nation, where the treatment of diseases and injuries, has not in some form obtained. Much importance has been attached to the assertion of Pliny the elder, that Rome had no physicians for the period of 600 years; as if this city were an exception to the general proposition just quoted. It is, however, the fact that in no age are physicians the sole practitioners; and I have also elsewhere shown that Pliny's

assertion is not correct.* The early physicians of Rome were merely slaves, both male and female; and therefore found no historian. This was the case indeed with the majority of them, until Cæsar decreed to all who practised medicine, the rights of citizenship.† In Athens, on the other hand, it was forbidden by law, that a slave or a woman should practise physic; and thus a dignity was imparted to our art which secured its transmission to succeeding times.

THE CONSTITUENTS OF A SCIENCE.

II. A *science* has been said to consist of the facts, ideas, and principles of any department of knowledge—classified and arranged in a systematic manner.‡

1. *Facts* are something external to ourselves, and therefore existing independently of our own minds; *e. g.* the rising and the setting of the sun. Facts then, depend upon the nature of things; and if actual or real, are, of course, true. But we acquire facts by the exercise of our senses. Whether we perceive facts, therefore, as they are, depends upon our powers of observation and their use. If these prove deceptive, our supposed facts are not true, *i. e.* they

* See *Amer. Med. Monthly* for April, 1859.

† Subsequently, however, slaves were again allowed to practise medicine. And the Code of Justinian, promulgated about A.D. 500, fixed the price of physicians, male and female, at 60 solidi—or $257 12.

‡ Sir William Hamilton's definition is too transcendental for our purpose. "A science is a complement of cognitions, having in point of form, the character of logical perfection, and in point of matter, the character of real truth."

are not real—are not facts at all, and are sometimes loosely termed "false facts." With facts, then, simple reality* is the test of truth; and no two facts can be contradictory, though our interpretations of them may be so. Any doubtful proposition, adopted as a fact for the time being, is a mere assumption, or supposition; or if used to account for certain phenomena it becomes a *hypothesis*. Theoretically, none but established facts should form part of a perfect science; but, during the formation of every science, hypotheses must be more or less used as a substitute, for the time, for actual facts, and till the latter can be verified. A single false fact being once generally adopted as true, may retard the progress of science for centuries; as did the assumption that the earth is stationary, that the planets revolve in circular orbits, and that the blood passes directly from the right ventricle of the heart into the left ventricle. The last assumption apparently deferred the discovery of the circulation of the blood for more than 1200 years.

2. *Ideas*, on the other hand, or notions, are the results of our own efforts of thought, and depend upon our intellectual operations. They are acquired by our understanding and reason, as facts are by our senses. They may be acquired from or suggested by facts; or indepedently of facts, or anything external to us—the imaginative faculty suggesting this latter

* In case of facts, reality is also the sole measure of possibility. "All that is real is possible; and all that is possible, exists." We cannot, then, assume in advance, that an alleged fact is impossible, unless the terms in which it is expressed include an inconsistency; as when a camel of the natural size is said to pass the eye of a needle—it being also understood to be of the ordinary dimensions.

class of ideas. Ideas, therefore, may be erroneous or incorrect, though the facts suggesting them may be true. *E. g.* anatomists before the time of Galen knew the fact of the existence of the arteries; but they held the idea that they contain air and not blood, and hence their name.

For scientific purposes, our ideas must be—1st, correct, or consistent with the facts suggesting them; and 2ndly, clear, definite and distinct. But the only demonstration of the *correctness* of an idea in natural science is the discovery of a new fact or principle corresponding in terms with it. Meantime, however, an idea must be received as correct, if it is well established; *i. e.* if it is consistent with known facts and generally received ideas, and is rationally adopted by adepts in the particular department of science to which it belongs. But ideas require to be reëxamined from time to time, and if possible, verified, even to a demonstration. But if it be said that the understandings of men differ to such an extent that we cannot be certain of the correctness of our ideas, while we may be of the reality of our facts, I remark, that the senses of men also differ in a scarcely less degree, and may therefore deceive, as well as the reason. The senses could never teach us that the earth revolves round the sun, but led all men, up to Copernicus' time, to adopt as a fact the assumption that the sun revolves round the earth. In science, therefore, the senses and the understanding have mutual need of each other. Supposed facts must be tested by reason; and ideas must be verified by the evidence of the senses As a general rule, we may

be as sure of the correctness of well established ideas, as of the reality of accepted facts, though the contrary is often asserted; observation being as often delusive as ratiocination.

When however, an idea is demonstrated to be correct by the discovery of a new fact corresponding to it, we cannot say that the idea has become a fact; it is merely proved to be correct. The idea existed, and still exists, in the mind alone; the fact is external. Leverrier had the idea, founded on reasoning to him quite conclusive, that there is a planet further than Uranus from the sun; and, pointing his telescope in its supposed direction, he discovered the planet itself. The fact now takes its place in the science of astronomy. But in case he had not made the discovery, the mere idea, sustained by such arguments as he adduced, and being well established by them, also deserved a place in his science, as a guide to a future discoverer. Correctness of ideas, therefore, implies, as Prof. Whewell remarks—"1, a basis of well known and clearly conceived facts; and 2, reliable reasoning, as based on invention, acuteness, and correctness of thought."

On the other hand, the *distinctness* and *definiteness* of our ideas may be quite independent of facts, or of truth.* We may form quite as clear and definite an idea of the sun's rising in the west and setting in the east, as of its actual course.

3. By a *truth* in science, is meant a proposition expressing the exact state of facts or things. The

* By truth is meant an exact conformity with the actual state of facts and things.

statement of an actual fact or a correct idea, is a truth. A *general truth*, or a truth including several minor ones of a similar kind, and logically deduced, is sometimes termed a *principle* in science. A principle is therefore not a general *fact*, as is sometimes asserted. All facts are necessarily individual and particular. It is simply a general proposition promulgated as true. But since if our ideas are incorrect our principles must also be erroneous, the latter must, like the former, be tested in every practicable way, before they should be accepted as well established, and admitted into science. A *law* in science, is merely a still higher generalization embracing several principles; or a still more extended general truth. *E. g.* Schleiden observed that the tissues of various plants are originally developed from cells; and hence deduced the law that all vegetable tissues are thus devolped. And subsequently Schwann extended this law to all animal tissues also.

For scientific purposes, therefore, our facts must be true—*i. e. real*—our ideas, correct and definite, and our deduced principles well established. Besides, all these must be classified and arranged in a systematic manner, before they constitute a science. But this last proposition is too evident to require illustration.

I should therefore say that "an actual science consists of the real facts, the correct and distinct ideas, and the established general truths of any department of knowledge, classified and systematically arranged." The more unquestionable the elements of any particular department of science are

from its very nature, the more it deserves the name of an *exact* or *positive* science. In this respect the pure mathematics takes the lead, since it concerns only the magnitude and the quantity of things. Since, also, its conclusions are absolutely certain, mathematics is termed *demonstrative* science, in contradistinction from the rest, which are termed sciences of probability. Natural philosophy and chemistry are regarded as exact sciences ; but just so far as the objects of a particular science can neither be measured, numbered, nor weighed, so far it is not capable of becoming an exact science. Medicine, therefore, with the exception to a certain extent of anatomy, cannot become an exact or positive science.

The division of the sciences into the *certain* and the *uncertain* is a mere contradiction. Uncertainty is associated with certainty in all the sciences of probability. But the human mind does not assent without evidence in one science more than in another, and accepts only that which it believes, on the best evidence attainable, to be certain—*i. e.* true ; though it sometimes, "like Ixion, embraces a cloud instead of a goddess."

Some writers assert that science consists of facts alone, since this term implies what is known, and we *know* only facts ; while our ideas and general truths, depending on our reason, may be false and unreliable. But we have seen that our facts, also, may be false ; and that, generally, our ideas and deductions are as reliable as our sensory perceptions. *E. g.* we feel as certain as to the law of definite proportions in chemistry as we are respecting any one

of the facts on which it is based. We may therefore say that science consists of *truths* alone, but not of *facts* alone ; and we have already seen that scientific truths include facts, ideas, and general truths. It should also be added that a fact has in *itself* no scientific value whatever. Its value consists entirely in the use we make of it, or the interpretation we give it. In other words, the mind alone gives it value. Proclus tells us the Epicureans used to say that even asses know the fact that any two sides of a triangle are together longer than the remaining one, and act accordingly ; but they are not, therefore, acquainted with the science of geometry. Many classes of facts are acquired by the lower animals ; but not being interpreted they fail to contribute to scientific ends. Facts, then, have a mere "potential or subsequent value, and the only advantage of possessing them is the possibility of rising by a mental process to the idea, the principle, or the law which governs them ;" or to the ideas in the Divine mind, which are shadowed forth by them. Facts are indeed the "body of science," as Dr. Samuel Brown remarks ; but the "ideas" and conclusions they suggest "are its spirit" and its soul. And "our real knowledge," says a recent writer, "consists not in an acquaintance with facts, which makes a pedant, but in the use of facts, which makes a philosopher."

But though facts are only the 'body of science,' while 'the ideas' and the conclusions they suggest are its soul, they are thus shown to be of primary and indispensable importance in the construction of a science —like the bricks or blocks of stone of which an edi-

ıice is constructed. But those who are satisfied with merely discovering and contributing facts, and leaving them for others to use, are the mere hewers of stone and the brick-makers, in science; while the master masons are those who assign to each fact its place and its value, and thus really build up the structure required. Some of the most distinguished philosophers and discoverers did nothing themselves towards obtaining the facts on which their discoveries were based. Newton, in a letter to Flamstead, remarked: "All the world knows I make no observations myself; and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown." Harvey did not himself discover a single one of the facts on which his discovery of the circulation of the blood reposed. To discover facts requires a high development and much practice of the perceptive faculties—of the senses; to deduce conclusions requires superior and well exercised reasoning powers; and these two diverse qualifications are but rarely blended in the same mind. Each is, however, of great value in its place, and both are indispensable to the advancement of science; though the latter performs the higher and the crowning function.

COMPARATIVE CHARACTER AND QUALITY OF MEDICAL SCIENCE.

III. In respect to the comparative character and quality of the science to which we are devoted, it has already been remarked that as a whole it is a science of probability, and, with the exception made,

cannot from its very nature become an exact or positive science. It is often repeated that medicine is an *inductive* science, and therefore to be classed with Astronomy, Physics, and Chemistry, and other mere physical sciences. But of the several distinct departments of medical science not one is exclusively inductive, and no two are equally so.

Since, however, they are all in some degree inductive, none of them but descriptive anatomy made any considerable progress till the inductive method of acquiring knowledge was distinctly recognized and began to be appreciated. It therefore becomes necessary to inquire what is meant by the inductive method, and when and by whom its application to the advancement of the sciences termed the inductive, was suggested.

THE METHODS OF SCIENCE—INDUCTION AND DEDUCTION.

I. An inductive science is one, it is said, which is advanced, or in which truth is obtained, by induction. What, then, is induction? or, to use a synonymous expression, what is the inductive process? Some would reply that induction is a peculiar process of reasoning; but this is not the fact. The peculiarity affects only the manner in which we obtain the *data* from which we reason, as will presently appear.

There are but two scientific methods of obtaining truth:—1st. To proceed from the external to the internal, from facts to conclusions, from effects to causes; 2d. The reverse of this—to proceed from

the internal to the external, from principles to facts, from causes to effects. The first is the *inductive* process, and is an *a posteriori* mode of reasoning; the second, the *deductive* or rational, or reasoning *a priori*. The inductive reasoner "rises from facts to the principle or law which governs them;" the deductive "draws the principle or law from ideas (not facts) already in his mind, and explains the facts by descending on them, instead of rising from them." The latter, therefore, necessitates a theory, or at least a general proposition, to start with; the former excludes theory entirely. The inductive method gives the first place to facts; the deductive, or rational, to ideas, or general propositions. Both alike include a reasoning process, neither of them being mere investigation;* and both are alike indispensable to the completion of every science, since each has its own peculiar mission to fulfill; and they sometimes follow each other as successive steps in the attainment of a particular scientific result. The reasoning process is the same in both; the "induction" consisting merely in the collecting and bringing into the account, one by one, the facts on which we reason. The inductive process, then, consists of—1st, the induction, as just explained; and 2d, the deduction—*i. e.*, drawing the conclusion from the facts. We therefore reason *from*, but not *by*, induction. We can never attain to certainty, but only to probability, by either of these methods. Hence, the con-

* Investigation includes the various methods of ascertaining facts, as observation, experiment, analysis, etc.

clusions arrived at by both require verification; which in the one case consists in repeating the induction in other circumstances, so as to collect a new series of facts; and in the other, in testing the conclusion by facts.

The same conclusion may sometimes be arrived at by both these processes, when they confirm each other; *e. g.* perceiving that a certain number of persons, apparently not similarly circumstanced in other respects, have alike a certain disease, and also work in lead, we may conclude *inductively* that all who work in lead are liable to the disease, and that lead is a poison. On the other hand, commencing with the idea, or theory, that all who work in lead have a certain disease, we may deduce the conclusion that lead is a poison. More frequently, however, the converse proposition is arrived at by the deductive process; *e. g.* from the idea or theory that lead is a poison, the conclusion is deduced that those who work in lead will become diseased. The reasoning in all these instances can be reduced to the form of the syllogism, as will be seen further on. But the deductive method has far the wider scope; since, while the inductive is confined to reasoning from a class of particular facts to a general conclusion, the other may deduce its conclusions from ideas, or theories even, or from single facts, or a number confessedly too small to justify an inductive conclusion.

Reliability of inductive conclusions.

The degree of probability of an *inductive* conclusion depends, so far as the *facts* are concerned, on three conditions:

1. They must be true or real, and therefore reliable.

2. They must be comparable—*i. e.* identical in kind.

3. They must be sufficiently numerous.

And, finally, these conditions being secured, the conclusion deduced must be logically correct. But this proposition applies equally to both the methods under consideration.

1. The *reliability* of the facts must depend—1, on our powers of observation, natural and acquired, since we obtain facts by our senses merely; and 2, upon our invention in the way of aiding those powers, whether by instituting the most favorable circumstances, as in experiments, or by using the best instruments. Repeated observation and experiment are therefore the test of the reality of our facts; "by the former of which," says Sir Gilbert Blane, "we may be said to listen to nature, and by the latter to question her." If our facts be false, our conclusions, if logical, must necessarily be erroneous.

2. But the facts adduced must also be *comparable* and therefore of the same kind. We cannot, on finding that of a certain number of persons working in lead one has colic, another typhus fever, a third pleurisy, and so on—there being as many different

diseases as there are individuals—deduce the general conclusion that this class of people is liable to colic, typhus fever, or pleurisy. Evidently, if we are to deduce any general conclusion in regard to a particular disease, the instances compared must all be instances of the disease in question. On the other hand, if we are to draw any general conclusion as to the effect of a particular calling or avocation, the instances adduced must all be examples of the same avocation.

Still, no one but an adept in the particular department of science can judge accurately of the precise nature and comparability of the facts pertaining to it. A physiologist alone can rightly collate physiological facts, preparatory to making a logical deduction therefrom ; and the chemist has the same exclusive judgment in regard to chemical facts. And neither can assume the place of the other without some risk to the science on which he trespasses, unless he at the same time thoroughly understands the latter also.

3. But the probability of a conclusion obtained by the inductive method also depends much upon the *number* of similar facts adduced. In general, we may say the number must be sufficient to warrant the conclusion. We must have enough instances to serve as a *sample*. But different minds will require different numbers to satisfy them as a basis for deduction ; and different departments of science are very diverse in this respect. In physics or natural philosophy, a single instance will sometimes (though rarely) warrant a conclusion ; in physiology and

pathology, many instances will be required; for in the latter it is more difficult to be sure we have ascertained all the circumstances which bear on the question, since the vital force modifies the action of the merely physical forces. The number of instances required in each particular case, then, is a mere matter of judgment; and here, also, only the adept in the particular science is a competent judge. The expression of this judgment forms the major premiss of the syllogism, and is based on a belief in the constancy of nature's laws; and whether the latter is or is not intuitive, makes no difference.

Such being the inductive method of acquiring truth, it is not singular that but very few can apply it advantageously for the advancement of science. For, 1st, there are always very few good observers and experimenters; 2d, of these but a small proportion can accurately discriminate and collate the facts they observe; and 3d, most good observers cultivate the perceptive faculties to the comparative neglect of the logical. Hence, they leave their facts quite barren of scientific results, till some logical mind completes the inductive process for which they afforded the materials, and thus makes a real addition to science. Bacon himself remarked that it is a method "difficult in its operation," though easily explained. There are, however, a very few, who can assign the true value to the facts they observe, and from them logically deduce the general conclusion. Such possess the really inductive mind, or that balance of the perceptive and the rational faculties which constitutes the good observer and the

good reasoner combined. With the deductive mind it however presents a very strong contrast. While it is plodding, cautious, non-committal and conservative, the deductive is impulsive, bold, frank, progressive and reformatory; the former is cold, rigid and matter-of-fact; the latter is congenial, not exacting, versatile and imaginative; the former is intensely and often exclusively practical; the latter is theoretical rather, though it can adapt itself to practical life. The inductive philosopher says, "I have shown it to be so;" the deductive, "It must be so," or "it is so, *a priori*." Children and most men are naturally inductive; poets, geniuses, and, I may add, most women, are naturally deductive, being more enthusiastic and emotional than the majority of men, and rather impatient of mere facts and details. Of the two great nations of antiquity, the Greeks were emphatically a deductive people. "In the art of experiment and trying to find his way with untripping step among details, the Greek was as feeble as a child; whereas, in the sphere of ideas and vast general conceptions, as well as in the fine art of embodying such universals and generalities in beautiful and appropriate symbols, it is not a paradox to say that he was sometimes stronger than a man."* The Roman mind was unscientific—and therefore had no decided deductive or inductive tendency—vigorous, stern, ambitious and practical. While Athens, therefore, was becoming the centre of refinement and civilization to the world, Rome was achieving conquests and mere material

* Eclectic Magazine, Oct. 1858, p. 198.

wealth; the former developed the individual, the latter looked mainly to the growth and the government of the state; from the former, therefore, we have received philosophy and science; from the latter, jurisprudence alone.

A comparison of the three most important nations of the present day, in this respect, is not without interest in indicating the part hitherto performed by each, and hereafter to be expected, in advancing our science—and would result nearly as follows: The English mind is intensely and almost exclusively inductive; and therefore slow, prudent, conservative and practical. The German mind is especially, but by no means exclusively, deductive; hence it is progressive, theoretical, and even transcendental, but less practical. The French mind is especially observant and adaptive—inductive or deductive, practical or theoretical, as may be required—versatile, revolutionary, and highly æsthetic. If I may also add the traits of American mind, I should say it is far more deductive than inductive, but is still practical; though restless, inventive and progressive. Compared with the preceding, it evidently operates under a "high pressure;" but it is capable of both observing and reasoning well, provided it can only *wait*.

The inductive method as proposed by Bacon.

II. The value of the inductive method of acquiring knowledge was first distinctly shown by Sir Francis Bacon, in his remarkable work entitled "Novum

Organum; or, True Suggestions for the Interpretation of Nature;" and which, after being twelve times re-written and revised, was published in the year 1620. His doctrines had been rudely foreshadowed in the writings of Roger Bacon, about 350 years before, (1270). "Each science," says Sir Francis, "has hitherto been a light and slender thing;" and he proposed a "reform in the system of human knowledge, by advancing a new method of using the rational faculties, and showing how to examine and interpret nature." He did not, however, himself term the new method the *inductive;* but in contrast with the method of the ancients, and especially the Aristotelian, he called the latter method "The Anticipation of the Mind," and his own "The Interpretation of Nature."

The "Organon" of Aristotle had exercised an almost exclusive sway over the human mind, in regard to the method of acquiring knowledge, for nearly two thousand years (since B.C. 340). It recognized logic as the fundamental science, and maintained that we cannot, without the strict test of logic, observe facts in their true light. Thus reasoning, with Aristotle, preceded observation; or rather all observation was tested by reasoning, before it could be accepted as reliable. He held, in his philosophy, to the principle of *experience—i.e.* that all our thinking must be founded on the observation of facts; but after all, facts could not be recognized as facts, unless they could be syllogistically established. Thus the *Aristotelian* has been called the "speculative philosophy"—it being mostly based on ideas, not

facts; and being a form of the deductive method already explained. For several centuries prior to Bacon's time, however, this method had been much abused. Certain theologians had, in the eleventh century, professed to represent Aristotle's method, under the name of Nominalists, in a controversy with the Realists, who believed with Plato, his master; and the contest was prolonged, each party alternately gaining the advantage, for about five hundred years, till the Reformation (1517). Thus, during the middle ages, "many ingenious but useless subtleties were passed off for logic and dialectics," under the great name of Aristotle; and the nicest distinctions in words were accepted for conscientious accuracy. This was by no means the fault of Aristotle, but it brought him into disrepute; and this state of the human mind in regard to the method of acquiring knowledge, still generally prevailing to Bacon's time, he sought to substitute the new or inductive method for the scholastic logic, and verbal dialectics, then so much abused and perverted from the Greek philosopher. The *Baconian* philosophy is therefore usually called the "inductive" in opposition to the Aristotelian or speculative; and his work was called the "Novum" Organon.

But Bacon did not assume that his "Organum" should ever after entirely displace that of Aristotle, and that we must always reason from effects to causes, instead of from causes to effects. Admitting the value of the deductive method in its legitimate applications, he wished to institute a new method, and as he believed, for the advancement of science,

a far better. But the inductive does not exclude the rational method, as so many have supposed; for all reasoning is one and the same process, as we have seen, and which may be clearly exhibited in the form of syllogism. Neither method alone is sufficient in all cases, or in any science, and each is invaluable in its proper place. Three errors, however, in regard to the *inductive method* have been quite prevalent, and have produced much confusion and retardation of science.

First—It has been understood to mean the process of investigation and of collecting facts; but investigation is merely a term to include observation, and experiment; is not a reasoning process at all, and, therefore, is not syllogistic.

Secondly—It has been held to denote the mere deducing of an inference from the facts collected. But this reasoning process is only a part of the inductive process, and also belongs equally to the deductive. Yet induction has also erroneously been regarded as a *distinct* kind of argument from the syllogism; though all reasoning may be syllogistically expressed.

Thirdly—It has been generally taken for granted since the time of Bacon, that certain sciences, called the inductive, are advanced exclusively by the inductive method; the deductive or rational method being in every respect to them entirely inapplicable. No science, however, is exclusively inductive; those so called are merely less *deductive* than *inductive*—*i.e.*, they are advanced to a greater extent by the inductive method than by the deductive. Even astron-

omy,* the most perfect of the inductive sciences, owes some of its sublimest discoveries to deductive reasoning. Newton's discovery of the law of gravity—the greatest of scientific discoveries—is far more the result of the deductive than of the inductive process.† —This last error has greatly arrested the progress of science, since the constant effort has been to advance the sciences termed inductive by induction alone. For more than two hundred years all deductive reasoning has been decried, and especially in England, as of no value in the advancement of science; while inductive reasoning has alone been blindly worshipped and encouraged. All sciences, however, require both methods for their advancement, as will appear.

* Kepler discovered his three great laws of planetary motion and distances, published in 1609, not by the inductive process, as is usually assumed, nor by the deductive, but by the tentative method of investigation merely; which consisted in starting an hypothesis, as he remarks, and then hunting it down. Some writers have, however, erroneously included this mode of investigation under the inductive method.

† Eclectic Magazine, June 1858, p.192, et seq. The three great questions which present themselves in our investigations in physical science, are indicated by the words, "What? Why? and How?"—*i.e.* 1. What is the actual phenomenon—the *fact*. 2. Why does it occur—the *cause*. 3. How does it occur—the *law*.

1. The *fact* of gravity, both terrestrial and celestial, might have been arrived at inductively from the fall of the apple, (if we accept the generally received anecdote,) provided the apple was regarded merely as a mass of matter. 2. The *cause* of gravitation was assumed to be a peculiar force, from its effects, and called the *force of gravity*. 3. But the *law* of its action—it varying "directly with the quantity of matter, and inversely with the square of the distance," was mainly a deduction, and which it required seventeen subsequent years of Newton's life to verify.

The deductive method also vindicated.

Since in the application of the deductive or rational method, we draw our conclusions from our ideas, while in case of the inductive method we reason in a precisely similar manner, but from established *facts* instead of ideas—it follows that in the formative epochs of all sciences, the deductive or theoretical must precede the inductive stage, from the paucity of established facts. Aristotle, therefore, lived too early to perceive clearly and to inculcate the inductive method; though he also had some imperfect idea of it in connection with the deductive method. But the deductive method again becomes indispensable when, in the progress of science, the inductive process has temporarily exhausted itself and is waiting for new facts to be discovered, sufficient to warrant new inductive conclusions. Several of the sciences are precisely in this state at the present time. I shall therefore vindicate the deductive, and even the theoretical spirit, so generally and so long denounced and discarded.

This servile worship of induction, and equally servile denunciation of all theorizing, has led to a blind admiration of mere observers, and a laborious accumulation of heterogeneous facts; and which must remain entirely worthless till the deductive mind gives them their true place and value. Hence, the man who finds a new chemical element or compound radical, though it were by mere chance, and though the discovery promises no benefit whatever to the race,

is, now-a-days, at once thought to be almost entitled to a monument; while Dr. Marshall Hall was, for years after his discovery, denounced by many of his profession in London, as a visionary theorist. We, as medical men, are also constantly reminded that "ars tota est in *observationibus*," and some of us thoughtlessly embrace the common error, and decry every thing in the way of scientific effort but mere observation. And yet we know that art does *not* consist wholly in observations; and, besides, that if it did, science does not. For though science cannot be formed or advanced *without observation*, we have seen that the observations of but few, comparatively, are of any scientific value; and that in all cases the value is bestowed on the facts acquired by observation, by the deductive mind which gathers them up and uses them.

The real ground of objection to the method employed by the ancients was, not that it was the deductive method—though it alone cannot accomplish everything in the advancement of science, as we have seen, and they employed no other definite method—but that in its application they reasoned from their *preconceived ideas*, and not from *correct* ideas, or ideas based upon actual facts. But this could not then be avoided, since they had not the facts, and knew not how to acquire them. But as science advances, the conclusions obtained by the deductive process become more reliable in themselves, and also more easily verifiable by ever accumulating facts. Besides, the objection does not hold against reasoning from correct ideas; and we have

seen that we may now very generally be as certain as to the correctness of our ideas as we are of the reality of our facts. It was in its application, therefore, that the ancients failed, rather than in the method itself, and for reasons which need not, at the present day, obtain.

When, therefore, I vindicate a return to deductive reasoning and theorizing, in addition to the inductive method, I mean not theorizing from ideas formed *independently* of facts, but from ideas *established upon real facts.* I also include deductions from facts insufficient to warrant an inductive conclusion. I insist, also, that all theories shall be verified before their promulgation, provided the necessary facts for their verification or refutation are afforded by the present state of science ; and, if not, that they be stated as theories merely, for future reëxamination. On these conditions, and these only, our theoretical conclusions deserve a place in science. Hence the greatest discoverers have ever been theorizers, and not mere observers ; though they, of course, recognized as indispensable the facts obtained by observation, whether by themselves or others. For every discovery, unless accidentally stumbled upon—as Glauber found the sulphate of soda while searching for the philosopher's stone—is but a theory in the mind of its author till he verifies it, and thus shows it to be strictly accordant with fact—or to be an actual discovery. But I shall again return to the subject of theories before I close.

I therefore conclude my remarks upon the inductive and the deductive methods in the words of Bacon

himself, though I here commend the latter more especially, by the terms he applied to the former. "Let there exist then, (and may it be of advantage to both), two sources and two distributions of learning, and in like manner two tribes, and, as it were, kindred families of contemplators or philosophers, without any hostility or alienation between them, but rather allied and united by mutual assistance. And as for those who prefer and more readily receive the former, let us wish that they may prosper as they desire in their undertaking, and attain what they pursue. But if any individual desire, and is anxious not merely to adhere to and make use of present discoveries, but to penetrate still further, let him, as a true son of science (if such be his wish), join with us; that when he has left the ante-chambers of nature, trodden by the multitude, an entrance may at last be discovered to her inner apartments." *

Medicine both an inductive and a deductive science.

Testing our own science by the preceding remarks, we find all its departments both inductive and deductive, though in some the inductive element very much preponderates. But we find that into two of them—Physiology and Pathology—a third element is introduced, which is unknown to the mainly inductive and the merely physical sciences. This is the "doctrine of final causes," or "teleology," as it is termed at the present day, and which will be ex-

* Nov. Organum, Præfatio, p. 3.

plained further on. The following arrangement of the various departments of medical science under three subdivisions, will illustrate the character of each.

1. Science of Structure,	Anatomy—various branches of.		Mainly inductive.
2. Science of Function,	Physiology and Pathology,		Inductive. Deductive. Teleological.
3. Science of Conservation,	Hygiene and Therapeutics,	Medical, Surgical,	Mainly inductive.

PART II.

THE PROGRESS AND THE SPIRIT OF MEDICAL SCIENCE.

Proceeding now to trace the progress of our science, its principal epochs are first to be designated.

It has been shown (p. 7) that art naturally, in point of time, precedes science. Certain rules or maxims, however, become current as art becomes more common and better understood; these being the first attempts at formularizing the results of experience, and preceding, by a long interval, any attempt to reduce what is known to a scientific form. These subsequently accumulate, and are expanded into definite precepts or aphorisms; then theories are superadded and conclusions deduced; and, finally, facts are obtained in sufficient number to justify the inductive process, and science at length begins to assume a definite form. To its completion, however, the deductive method still continues to be as indispensable as the inductive.

Auguste Comte states, in his "Philosophie Positive," that all sciences must have three principal epochs of development: 1. The childish religious; 2. The boyish metaphysical; and 3. The manly positive. I have shown (p. 13) that ours is not, in any philosophical sense of the term, a positive science; and in tracing its progress I shall adopt as its three principal epochs, 1. The superstitious; 2. The epoch of unsystematized medical learning; and 3. The epoch of systematized learning, or actual science. The first two epochs are of course the formative periods of our science, and during the second it consisted mainly of theories, aphorisms, and monographs. There are also transitional and stationary, and even retrogressive, periods; but these need only incidentally detain us. I shall also pass rapidly through the two formative periods—the superstitious and the unsystematized—and consider more especially the more recent and the present condition of our science—its systematized or actual state. The period of time covered by each of the epochs just mentioned is seen below.

1. Superstitious epoch—about 3600 years.	From the earliest times to Hippocrates, or about B.C. 400.
2. Epoch of unsystematized learning—about 2000 years (theoretic, aphoristic, and monographic epoch).	From Hippocrates (B.C. 400) to Harvey (A.D. 1620).
3. Epoch of systematized or actual science—about 240 years.	Since Harvey (A.D. 1620) to the present time.

I.—THE SUPERSTITIOUS EPOCH.

The superstitious epoch extends over a period of about 3600 years—or from the earliest times to

Hippocrates (about B.C. 400 years). It has been divided into the primitive and the mystical periods; the former ending, and the latter commencing, with the Trojan war, B.C. 1184. Of the medical ideas of the antediluvians we have no reliable information. It is however pretty certain that their knowledge was preserved in Egypt, the only portion of the world which was not for a long time after the deluge, in darkness. Still later also, the most enlightened nations were the Egyptians, the Babylonians, and the Phœnicians.

Of the Babylonians, we only know that they had the custom of carrying their sick to the public squares, that the passers-by might suggest such remedies as they had known to be beneficial in similar cases. What their ideas of disease were, we have no means of knowing. The Phœnicians were formerly the Canaanites, the descendants of Ham; and were conquered by the Hebrews, the descendants of Shem, after the latter went out of bondage in Egypt. The latter did not add to the learning obtained from Egypt during their sojourn there; and medicine is least of all the professions indebted to them. And as the art of medicine in Egypt consisted mainly, as will appear, of religious rites, and incantations, even King Solomon was, among his countrymen, as much distinguished for his knowledge of charms and incantations, as for his general wisdom. He was also a necromancer of great power; and his use of supernatural agencies in the cure of diseases is yet celebrated in the traditions of the oriental nations. As instances of the practice of the Jews, I will remark,

that the egg of a grasshopper and the teeth of a fox were employed by them as enchantments, especially in diseases of the mind. But to return to the Egyptians. They made very little addition to the knowledge they had received from the antediluvians.* In Egypt the occupations were hereditary, and the professions were restricted by law; which prescribed the precise information, facts, and principles, of which the science of medicine should consist. The only medical book then extant, however, of which we have any account, is a treatise on anatomy, by Athothis, the son of Menes, one of the Egyptian kings. All improvement by experiment was entirely prevented; for if the physician departed in the least degree from the practice laid down in the books, and the patient died, his own life was forfeited. Each physician was also confined in his practice to a single disease; and human dissections were not permitted.

The practice of our art must at all times be based more or less upon the prevalent ideas of the causes of disease; and it is natural for the human mind to refer what is entirely inexplicable, to supernatural agencies. Disease was therefore at first attributed to such influences; and hence its treatment consisted principally in charms, incantations, and religious ceremonies; these being also used quite as much with the intention of averting, as of curing, diseases. Sometimes even human sacrifices were offered to appease the supposed anger of the gods.† These ideas

* The deluge occurred A.M. 1656, or B.C. 2348.

† Long after those times, seven young men were annually sacrificed at Athens for this purpose.

of spiritual influence were also encouraged by the priesthood, who exerted a controlling influence over the popular mind.

But Egypt, we have seen, was merely the preserver, not the mother of the sciences, so far as they were then understood. And having at last fulfilled her mission in this respect, she transmitted her cherished treasures to the Greeks. The latter were a mixed people, principally composed of Egyptians, Phœnicians, and Phrygians. Into Greece therefore, the learning of the Egyptians was readily introduced, and there took the form of science and civilization. Composed of such varied intellectual elements, the Greeks were a versatile, active, speculative, and progressive people;* experimentation was not prevented by law as in Egpyt, and our own art and science soon received important additions.

It was also believed by the ancient nations, that certain substances possess a health-preserving power; as the jasper, and some other precious stones. These were therefore worn as amulets; and thus we discover the original use of many of the trinkets and ornaments worn at the present day.

In Chaldea, as well as in Egypt, the heavenly bodies were also supposed to exert an influence upon diseases; and hence astrologers were consulted to avert them. The notion still extant in some parts of this country, that a child must be weaned, or have its ears punctured, only when the *sign* is right, is a relic of this very ancient pathology. In the middle

* Hence in Greece the most honorable titles were expressive of wisdom and knowledge, rather than of political station or wealth.

ages a somewhat related idea, obtained in Europe, viz.: that the remedies for particular diseases are foreshadowed by their physical properties, as their form, color, etc.; and this has been called the "doctrine of signatures;" *e. g.* the celandine was accepted as a remedy for bilious derangements, because its juice is yellow and viscid, like the bile; the liverwort, also, was thought beneficial in liver diseases, and the lungwort in those of the lungs, from the shape of their leaves. Some analogies were carried still farther, even to a figurative signification; and the teeth of serpents, reduced to a powder, were prescribed to despairing lovers, that they might thus acquire the power to fascinate the other party.

Of the ideas of medicine entertained in Greece in the earliest times, we have no knowledge; but may safely assume that they were entirely derived from Egypt. The first Greek physician of whom we have any account, was Melampus, who cured the daughter of King Proteus with black hellebore; and which is named from him, "Melampodium." Chiron, the Thessalian, was the next. He was not, however, a practitioner, but a philosopher; and merely taught the science of medicine, as then understood. And here the *primitive* period ends.

Æsculapius flourished during the Trojan war—(B.C. 1184), when commences the mystical period of the superstitious epoch. He was by the ancients esteemed the father of medicine. Much, therefore, that is mysterious and fabulous, in addition to his asserted origin from the gods, is associated with his name. Cicero says he invented the pulling of teeth. His

sons, Machaon and Podalirius, were also famous as advancers of our art and science. But after the Trojan war, and the beginning of the troubles between Greece and Persia, medicine again degenerated for nearly 800 years, into mere superstitious rites and the interpretations of oracles. And thus closes the *mystical* period and the superstitious epoch of our science.

II.—EPOCH OF UNSYSTEMATIZED LEARNING.

The second epoch of our science—that of unsystematized knowledge—begins about B.C. 400, with Hippocrates, and extends over a period of about 2000 years, to the discovery of the circulation of the blood by Harvey in 1620. This is subdivided by Renouard into four periods.

1st. The *Philosophic*, which he dates however from Pythagoras, B.C. 500, to the foundation of the Alexandrian Library, B.C. 320.

2d. The *Anatomical*—from last date to the death of Galen, A.D. 200.

3d. The *Transition* period—from Galen to the revival of letters, A.D. 1400.

4th. Period of *Renovation*—A.D. 1400, to Harvey, 1620.

The Philosophic and Anatomical periods are, however, only two phases of the theoretical or monographic; the transition, is only a stationary period; and the period of renovation, the theoretical period again returning with a new impulse.

The works of Hippocrates.

This épоch was really initiated by the labors of Hippocrates, about the year B.C. 400; and to which the teachings of Pythagoras, and his followers about 100 years before, must be regarded as the prelude. Pythagoras, however, merely taught what was known of medicine, as a philosopher, not being himself a physician. Hippocrates removed from medical knowledge its uncertainty and its mystery. Pythagoras had shown that the labors of Æsculapius were but human; Hippocrates taught his contemporaries rightly to appreciate their value, and first gave to medicine a cyclopedic, or rather a monographic form. Of the many works attributed to him, perhaps the following only can, without *any* doubt, be admitted to be genuine; though it does not comprise one-fourth part of the Hippocratean collection.

The Prognostic.
The Aphorisms.
The Epidemics—(Lib. 1st and 2d.)
On Regimen, in Acute Diseases.
On Airs, Waters, and Places.
On Articulations, or Luxations.
On Fractures.
Treatise on Instruments for Reduction.

These works, considering the time when they were written, are remarkable for their practical value, and for nearly 1800 years exerted a paramount influence on medical men. But they are now found to abound in mere theories, and to contain very little true science.

Most, even of the aphorisms, so long blindly extolled and admired, as the sublimest effort of medical genius, and a certain number of which the faculty

of Paris, till within a few years, required all candidates for the doctorate to insert in their theses—are now found to contain "exceptional truths, vulgar reflections, and even errors and contradictions." (Renouard, p. 116.) Hippocrates was, however, everywhere accepted as a supreme authority, in medicine, up to the 16th century, though somewhat objected to by Galen, 600 years after his own time; and the science and the art of medicine have been transmitted to modern times by him.

The foundation of the Alexandrine Library, about forty years after the death of Hippocrates (or, B.C. 320), gave a great impulse to medical studies. There Erasistratus and Herophilus taught, and somewhat advanced the science of anatomy; but their works are lost. The principal works of which we have any remains, for 500 years after Hippocrates, are the two books of Aretæus, on semeiology (about A.D. 200); the treatise of Cœlius Aurelianus, on chronic diseases; the eight books of Celsus "de Medicina;" and the works of Galen on hygiene, anatomy, and physiology.

The time from the foundation of the Alexandrine Library (B.C. 320) to the death of Galen, in A.D. 203, has been designated as the *anatomical* period. And from this date to the discovery of the circulation of the blood, in 1620, a period of 1400 years, and extending through what I have before termed the stationary, and the return of the theoretical period, but slight additions were made to our science.*

* See Appendix, note I.

Medical sects.

Several sects arose during the anatomical period, which should be mentioned here. 1. The *Dogmatists* trusted to theory, assuming to be the followers of Hippocrates. 2. The *Empiricists* (Empirics), who adopted the opposite extreme of relying on observation and experiment alone. 3. The *Methodists*, who combined theory with experience. 4. The *Pneumatists* (or Spiritualists), who asserted the influence upon the body of an ethereal element, or the animal spirits. 5. The *Eclectics*, the last sect before the dark ages, who professed to select and combine all that was valuable, from the preceding. But none of these made any essential addition to the theories of Hippocrates. And during the 1200 years ensuing after the death of Galen (*i. e.* A.D. 200 to A.D. 1400), we find only commentators upon, and servile imitators of, Hippocrates and Galen ; the art and the science of medicine having, throughout Europe, during this period, again relapsed into the hands of the priests. Among the Arabs alone medicine was still preserved, after the destruction (A.D. 640) of the Alexandrine Library to A.D. 1400. But the Arab writers are also scarcely more than mere commentators on Hippocrates and Galen.

From A.D. 200 to 1400 is, therefore, the stationary period of our science, or rather the *retrograde* period ; for it actually retrograded, instead of advancing—except in Arabia, where it barely maintained its former condition.

But if we must admit that the medical mind was entirely servile during that long, dark period, *all* mind was in the same condition; and no important advance was made in any science. For the writings of Roger Bacon, published about the year 1260, on various departments of science, were two or three centuries in advance of his age; were regarded as heresy, and therefore suppressed, and he was imprisoned on their account.

During this period, and indeed for a century afterwards (to 1500), skepticism was as rare, and intolerance as severe, in philosophy and medicine as in religion; and the "infallibility of Aristotle,. Hippocrates, and Galen, was as implicitly admitted, as that of St. Paul and St. Angustine." Hence it was that so many medical errors were taught and defended with entire conviction, by minds of the loftiest intelligence. In the present age of freedom of thought and expression, this seems truly astonishing.

During the 14th century, however, the spirit of religious freedom and commercial enterprise revived the cultivation of the arts and sciences in Italy; and in 1315, Mondini, Prof. of Anatomy at Bologna, first publicly dissected two human bodies, and is regarded as the father of human anatomy. On all anatomical questions, however, medical men were still accustomed to swear by Galen, even to the time of Harvey.

From 1400 to 1620 (termed the *erudite* period by Renouard), great progress was made in anatomy by the labors of Fallopius (1560), Eustachius (1570), Vesalius, Fabricius, Cæsalpinus, and others; and the

absolute authority of Galen began to wane. The great work of Vesalius, on the structure of the human body, was published in 1543; and in the same year Copernicus published his discovery that the sun is the centre of the solar system, which "fell like a bomb" upon the stagnant intellect of the time; and in the last part of this period (about 1600), the early writings of Bacon had been published. This period is, therefore, the prelude to the last epoch of our science—its systematized or actual state—which commenced with the discovery of the circulation of the blood.

It was during the first part of this period that the separation of medicine from the priesthood, commenced in the 14th century, was consummated; and during the last century of it (the 16th) celibacy ceased in France to be obligatory on physicians, and they no longer obtained ecclesiastical benefices.

But a revolution at once from superstition to true science was impossible; there must be—1st, a denial of the doctrines before blindly admitted; and, 2d, a substitution of something entirely new, and which should seize at once upon the imagination, in order, secondarily, to interest the intellect. It was, therefore, during the 16th century that the "occult sciences," as they were called, engaged so much attention, and were applied to medicine; and which were the most remarkable forms of pseudo-science the world has ever seen.

The occult sciences.

The first step towards the revolution—the denial of the doctrines hitherto implicitly believed in—was taken by Cornelius Agrippa, about the year 1520, in his work on the "uncertainty, vanity, and abuse of the sciences." In it he maintained: "That there is nothing more pernicious and injurious to common life, and nothing more pestilential to the salvation of souls than the arts and sciences." He excepted only laborers and shepherds. Even alchemy, which he had practised a good deal, he berates in the following terms: "In fine, having lost the time and the money which you have devoted to it, you will find yourself old, ragged, hated, famished, always smelling of sulphur, soiled with sweat and charcoal, paralytic with frequent manipulations of quicksilver, and gaining nothing but a running nose; in a word, so unhappy that you will be willing to sell your body and even your soul."

The "occult philosophy," so called, was divided into four branches. 1st. *Theurgy* or theosophy, to which a man raises himself by prayer, meditation, and ecstasy; and which enables one to produce supernatural phenomena, by the intervention of God. 2d. *Magic*, or the art of controlling demons, and, through them, imitating true miracles. 3d. *Astrology*, or the art of reading future events, in the case of individuals, nations, or diseases, by the stars. And 4th. *Alchemy*, the object of which was to find—1st, the philosopher's stone, which should transform the other metals into gold; and 2d, the elixir of life,

which should cure all diseases. The practice of these was termed the "cabalistic art," and their application to medicine was termed cabalistic medicine. Agrippa was a cabalistic physician.

The second famous cabalistic doctor was Jerome Cardan, who flourished from 1530 to 1576. Paracelsus was, however, the boldest, and the most influential of them all; his career extending from 1525 to 1541. He quarrelled with the whole medical faculty, and praised the chemists and the alchemists, though both disowned him. He introduced antimony into the practice of medicine, and was the father of spagyrism, or the use of chemical remedies. He professed at last to have discovered the elixir of life, and died at the age of 48, with a bottle of it, as is said, in his pocket.

Soon after Paracelsus' death, the ideas of the chemical sect he had founded, who held that all the functions are mere chemical action, were exploded. But medical science, in its early doubting state, always took its phases, for the time, from the prevailing intellectual direction. Hence, as in the 15th and 16th centuries, it turned to alchemy, and sought the elixir of life, in the 17th it leaned for support upon mechanics and mathematics; and hence arose the sect of iatro-mathematicians—who, led by Borelli (1690), assumed that the human body is a machine, and all its functions are due to mechanical force. But Hoffman, about 1720, recognized the influence of the nervous system and the vital force; and thus exploded Borelli's system, and laid the foundation for the true science of physiology and pathology.

Medical doctrines.

Subsequently, however, to the adoption of the Baconian method, we find no more medical sects; but the elements of our science were progressively consolidated into an indivisible unity. Particular *doctrines* have been put forth, from time to time, but with very little influence on its progress. Such were 1. The *chemico-theosophical* doctrine of Van Helmont (about 1610), who, though a chemist, maintained that the functions of the body are controlled by an immaterial principle, which he termed the Archæus. 2. The *chemico-material* doctrine of Sylvius (1690), who held that all diseases are due to chemical changes, producing an excess of acid or of alkali. 3. The *psychiatric* doctrine (about 1710) of Stahl, who, denying all chemistry, asserted that the living soul is the only moving force in the human body. 4. The doctrine of Solidism—*i. e.* that diseases are located in the soft solids, especially the nerves, proposed (about 1778) by Cullen; for all had been humoralists to his time. 6. The doctrine of Brown, that all diseases are sthenic or asthenic. 7. And last of all, Hahnemann's doctrine of "*similia similibus curantur*," infinitesimal doses, and the psoric origin of diseases. All these doctrines, but the last, have been entirely exploded; and this, as announced by Hahnemann, has nearly or quite died out in the land of its origin, and still retains a distinct name among us also, only by gradually approximating more and more nearly to the established principles of our science.

State of medical science in Bacon's time.

Taking now our standpoint with Bacon—the time being 1600 to 1620—we must admit with him that our science, like all the other sciences, had hitherto been a "light and slender thing." Little had been added to the science of conservation since the writings of Hippocrates, Celsus, and Galen. Instead of acquiring facts as a basis of advancement, physicians had given their attention to generalities, to the neglect of nature, though the "philosophies" whereon they depended were trifling, and "medicine not founded on philosophy is a weak thing." Hence, he considered that our art at that time stood among "the most conjectural ones," and that "great pains had been bestowed upon the cure of diseases, but to small purpose." He adds, "medicine, therefore, has been rather professed than labored, and yet more labored than advanced, as the pains bestowed thereon were rather circular than progressive; for I find great repetition and but little new matter in the writers of physic."*

A rapid glance, therefore, at the recent additions to anatomy and surgery, at some of the inventions of the two preceding centuries, and at the rude physiology of the circulation which then existed—must introduce us to the third and last epoch of our science, which dates from the great discovery of Harvey.

* De Augmentis, lib. iv. cap. ii.

I. Anatomy, though having received an impulse from Mondini, as already explained, in 1315, first acquired any important accessions from Vesalius; and during the century preceding Bacon (1500 to 1600), it had been advanced by him and Varolius in Holland, by Sylvius in France, and by Fallopius, Eustachius and Fabricius in Italy. Parè, the father of French surgery, had also taught to heal wounds by the first intention, had revived the use of the ligature to arrest hemorrhage, and first used the twisted suture in hare-lip. Tagliacotius had invented the rhinoplastic operation; and Fabricius the modern trephine and the use of the tube after tracheotomy. Wiseman, the Parè of England, was the contemporary of Bacon. Sanctorius had called attention to the cutaneous and pulmonary transpirations; and Servetus had suggested the pulmonary circulation.

II. Several important inventions had, however, been made during the 15th and 16th centuries, in accordance with a previous statement, that art naturally precedes the science on which it is based. Of these, I specify only a few. The 15th century had produced the invention of oil painting on canvas (1410), wood engraving (1423), printing (1442)—paper having been used over a century previously (since 1312)—the establishment of the post-office (1464), and aquafortis and line engraving (1480, twelve years before America was discovered). As belonging to the 16th century, the following inventions may be noted. The pocket-watch (1500), the spinning-wheel (1530), the padlock (1540), the use of pins

(1543), sealing-wax (1550), public carriages (1571), bombs and mortars, and telescopes (1588).*

III. The notions of the physiology of the circulation entertained by the medical men of Bacon's time included three fundamental errors, which demand some explanation in this connection.

1. The arteries were supposed to contain air and blood ; the air passing into them from the trachea.

2. The two ventricles of the heart were believed to communicate, by holes through the septum which divides them.

3. The veins were supposed to carry the blood to the various parts of the body.†

1. The ancients held that the arteries contain air only, but Galen had shown that they contain blood ; still maintaining that they also contain air, which enters the blood to cool it, from the trachea, and then passes out again. The fact that the blood contains oxygen could not of course be known till after the discovery of the latter by Priestley ; so that the discovery of the aëration of the blood, as now understood, is of but recent date. Galen, therefore, termed the blood in the arteries "spirituous blood," and held that it nourishes the lungs and other delicate organs ;

* Entering upon the 17th century, we find that glass* was first manufactured in France in 1608; naval telescopes in 1610; barometers and thermometers in 1620; printing of colored engravings, 1626; the pendulum, 1636; the streets of Paris were first lighted and swept in 1666, the year of the plague in London; the first opera in Paris, 1669.

† Flourens: Histoire de la découverte de la circulation du sang. Bulletin de l'Académie de Médecine, Paris, 1855, tom. xx. p. 648 et suiv.

* Plate glass was not made till 1688.

while the venous blood nourishes the coarser organs, such as the liver.

2. So deep was the conviction for 1300 years, of the communication between the two ventricles, asserted by Galen, that anatomists at length actually *saw* the holes they believed in. But at last, Berenger de Carpi, who preceded Vesalius as Professor of Anatomy at Padua, ventured to admit that they are seen with *very great* difficulty ("cum maximâ difficultate videntur"). But not till 1543 was this error overthrown by Vesalius, who, daring to use his own eyes, positively asserted that there is no such communication as Galen had described; and thus, for the time being, excited the animosity of the whole medical profession against him.

3. Michael Servetus, not a physician but a theologian, had, in 1566, made a lucky guess, and asserted that the venous and arterial blood are mixed in the lungs; or, in other words, had asserted the pulmonary circulation, in a theological work entitled "Christianismi Restitutio." He was, however, burned as a heretic, six years afterwards, and since only two copies of his book escaped the flames, but little importance was attached to this correct assertion, blended as it was with many physiological absurdities. The pulmonary circulation was, however, reasserted by Realdo Colombo, a pupil and successor of Vesalius at Padua, in 1572; and by Cæsalpinus, the botanist, in 1593. The latter was the first to use the phrase "circulation of the blood."

But of the *general* circulation, no one but Cæsalpinus had yet even a suspicion. It was now believed

that the arteries commence in the heart and the veins in the liver, and that the latter carry the blood from the liver to every part of the body. Fabricius had discovered the valves of the veins in 1574, and noticed that they point towards the heart; but this he supposed was merely to prevent the blood from unduly accumulating in the lower parts of the body. The barber-surgeons must, of course, have seen that on applying a ligature to the arm, preliminary to venesection, they must have stopped the course of the blood through the veins *towards* the hand; and, therefore, that the venous blood, which flowed, must have been flowing from the hand and towards the heart. But they merely *saw*, they did not give any value to the fact (p. 14).

But even the pulmonary circulation was not rightly understood by Cæsalpinus and his two predecessors, just mentioned; so that, *as understood*, it was a barrier rather than an aid to Harvey's discovery. They had no idea that *all* the blood passes through the lungs, but only so much as was necessary for the reception of the *vital spirits*—*i. e.* the same quantity which Galen had supposed to pass from the right to the left ventricle through the holes in the septum.

Harvey's discovery of the circulation.

Such then were the ideas of the circulation when Harvey commenced his investigations. Born in 1578 (four years after Fabricius' discovery), and being fourteen years the junior of Galileo, he became the pupil of Fabricius, then Prof. of Anatomy at Padua, about

the year 1615. His attention was directed to the discovery of the valves of the veins by his preceptor, and the question occurred whether the blood does not, after all, pass through the veins *towards* the heart, instead of the opposite direction. He instituted experiments in 1616 to 1618, in order to decide that question; and in 1619, being then 41 years of age, he first publicly taught his discovery of the true circulation of the blood. Thus, Fabricius' discovery had lain a barren and useless fact for forty-five years.

When this grand deduction—for it was not an *induction*—was once made, it seems so simple that we are now astonished that it was not made before, or that it found such violent opposers. Aristotle is, indeed, said almost to have discovered the true circulation, nearly 2000 years before. But there are in the history of all the sciences "stray passages of meaningless vagueness, wherein older authors indicated something like the truths afterwards established on the firm basis of experience and reasoning;" and we award to Aristotle, as to Servetus, merely the merit of a lucky conjecture. Very likely both would have opposed Harvey's discovery, had they been living at the time. Indeed, an extreme difficulty is experienced, even by the most eminent men, in seeing the plainest facts when they contradict all their preconceived notions, and are opposed to the dominant opinions of the age. And we have seen that the most striking characteristic of the human mind, during the preceding 1300 years, had been an unquestioning and acquiescent servility. No one, then, but

a man of genius could have coördinated and interpreted the discoveries of the preceding half century; and no one but an independent thinker could have made and defended the deduction therefrom. Others were on the *verge* of Newton's discovery of the law of gravity, but he alone had the talent to make it. Harvey did not publish his discovery till nine years afterwards (1628). His ideas were, for the most part, readily adopted by his countrymen; but abroad he was attacked from all quarters as a mere theorist; Riolan, a most famous teacher of anatomy, and professor in the College of France, being his most violent and eminent opponent; while Guy Patin employed his caustic satire against him; and Molière and Boileau ridiculed the whole faculty, thus disagreeing among themselves. Descartes, Swammerdam, Sylvius and Malpighi, however, adopted the discovery with enthusiasm. Harvey replied to his opponents in a spirited but dignified manner, but retained an unpleasant recollection of his opposition. He himself admits that after the publication of his work his practice essentially diminished, he being thought a crazy visionary. After the death of his patron, Charles I., and during the civil war, his house was plundered in his absence of all his furniture, and of what he most of all valued, the records of his experiments. He also lost all his labors on the generation of insects. He published his work on the Generation of Animals in 1651; and to Harvey we also owe the first announcement of the law "Omne vivum ex ovo."

Thus Harvey discovered the *fact* of the circulation

of the blood. He, however, could not discover the whole *course* of the circulation, since he could know nothing of the capillaries. Neither did he understand the function of the lacteals, discovered by Aselli,* in 1622; or of the receptaculum chyli, discovered, in 1648, by Pecquet. He maintained that the lacteals contain milk and not chyle. He supposed the office of the lungs is to cool the blood, and prevent it from boiling up. He supposed the blood passes from the arteries to the veins in two paths: 1st, the two kinds of vessels (arteries and veins) directly communicating; and 2d, through porosities of the part.

The simple microscope was invented in 1621, but the capillaries were not discovered till 1661, four years after Harvey's death, by Malpighi. Leuenhoek also described the capillary circulation in 1668, as if it had been previously unknown. And thus the *course* of the circulation was demonstrated. Bichat was, however, the first to recognize the capillaries as a distinct system, nearly sixty years ago; and Henle first investigated their structure so recently as 1841. The *cause* or forces of the circulation could not be ascertained till histology took a scientific form, and the precise action of the different parts of the circulatory apparatus had been ascertained. Harvey, how-

* The lacteals had been discovered by Erasistratus about B.C. 300, and were afterwards forgotten. Aselli's work, printed at Milan in 1627, is the first containing colored anatomical figures—the arteries and veins being in red, and the lacteals in black. It was supposed that the lacteals carry the chyle to the liver, till Pecquet traced it to the thoracic duct and the subclavian vein.

ever, described the motions of the heart far more accurately than most subsequent writers.*

The 240 years which have already elapsed since Harvey's discovery have not sufficed to complete it, and to demonstrate all its relations and its importance to our science. It is not strange, therefore, that we have no very important advance to report in our science during the remainder of this century —*i. e.* up to 1700—in consequence of it. Forty years had passed after its promulgation before all opposition to it had ceased, and its true value began to be considered; "a certain succession of time and of persons being generally necessary to familiarize men with one thought before they can advance to that which is next in order." (Whewell.)

I should not, however, omit the following names of eminent men in our profession who honored it in the different countries of Europe, and improved our science by their labors, in the 17th century. In Great Britain—Sir Thomas Browne; Dr. Darwin; Wiseman, the father of British surgery; Sydenham, the English Hippocrates; Lower, who first suggested the idea of transfusion of the blood; Wharton, Willis, and Glisson, whose names are incorporated into the nomenclature of the science they advanced; Drs. Goulston and Radcliffe, and the celebrated John Locke. In France were Pecquet, Vieussens, and Tourneforte. In Germany, Wirsung and Schneider. Stenon in Denmark. In Holland this period was

* See an account of Harvey's discovery in Blackwood's Magazine for July, 1858.

distinguished by the names of Bidloo, Graaf, Ruysch, the best of minute anatomists, and who sold his museum, made by himself, to the czar of Russia for 30,000 florins ; Nuck and Tulp ; Leuenhoek, the originator of microscopical anatomy, though not a physician ; and Swammerdam, the most ingenious of all dissectors of insects. In Italy, I can only mention Bellini, Borelli, Malpighi, distinguished in minute anatomy, and who first saw the circulation under the microscope ; Lancisi, physician to three successive popes ; and Ramazzini, distinguished for his varied learning.

Advancement of physical science.

Meanwhile, however, in *physical* science, which had presented an almost entire blank for 1000 years after the fall of the Roman empire (A.D. 400)—and especially in astronomy—an astonishing advance was being made in the 17th century. Galileo made his telescope in 1609, and applying it to the heavens, almost at once discovered the moons of Jupiter, and confirmed by direct observation the doctrine announced a century before by Copernicus. He also invented the pendulum, in 1636; the idea having been suggested to him while a medical student, on seeing a chandelier swinging in a church at Pisa. Kepler published his discovery of the laws of the planetary motions and distances, in 1609. Newton, born in 1642, discovered the law of gravitation—the most splendid scientific deduction ever made—in 1665, or 46 years after Harvey's discovery; and his Principia

were published in 1687. The science of mechanics had also been greatly advanced meantime, especially by Galileo's discoveries.

III.—EPOCH OF SYSTEMATIZED OR ACTUAL SCIENCE.

Thus it appears that though the third epoch of our science dates from the discovery of the circulation of the blood, about 1620, it may for all practical purposes be regarded as commencing with the last century, when the influence of that discovery began to be felt; and when it became finally emancipated from the control of systems and doctrines, and rapidly assumed a systematized form. It must serve my purpose to give a very brief abstract of its progress, therefore, during the last 150 years, under the three subdivisions before mentioned. (p. 32.)

Progress of anatomy since 1700.

I. The science of *descriptive anatomy*, has been advanced since 1700, by the labors of Cheselden, John Hunter, the Monroes, the Bells, Astley Cooper, Grainger, Sharpey, and Quain, in Great Britain; of Petit, Bichat, Cloquet, Cruveilhier and others, in France; Malpighi, Scarpa, Galvani, and Pacchioni, in Italy; Albinus, and De Haen, in Holland; Schmucker, Zinn, Meckel, Soemmering, and Rosenmuller, in Germany; and by Winslow, a Dane.

General anatomy was originated by Bichat, who published his great work in 1801; it having been written within a single year. He first attempted to

reduce the complex structures of the body to their peculiar tissues, and to introduce a systematic order into the study of anatomy and physiology, not known before his time. Upon the basis laid down by Bichat, the science of histology, or minute general anatomy, has arisen during the last fifteen years; and which, advanced by the labors of Schwann, Todd and Bowman, Queckett, Kölliker, Robin, and many others, at length underlies the whole domain of anatomy, physiology, and pathology; and promises the grandest results, as the starting-point of future discoveries in the departments just mentioned.

Microscopical anatomy may be said to have originated with Leuenhoek, about the year 1668. But it made but slow advancement, on account of the defective instruments used. Great improvements were however made in the construction of the compound microscope in 1832, and since that time it has advanced at a rapid rate.

Comparative anatomy, or the study of the structure of the lower animals, was the first department of anatomy in point of time. We have seen that dissections of the lower animals only were allowed by law, till the time of Mondini, or about five hundred and forty years ago. After human dissections were permitted, comparative anatomy was neglected, till again resorted to by naturalists, and especially by Cuvier, as a basis of classification of the animal kingdom. It was also studied by Hunter, as a basis of general physiology, and is still cultivated by many in all the countries of Europe, in its relations more especially to human structure and function.

Pathological anatomy was initiated near the end of the last century, and has been advanced by the labors of Hunter, Baillie, Carswell, Hodgkin, Abercrombie, Cooper, and Paget, in England; Bichat, Pinel, Andral, and Cruveilhier in France; by Engel, and above all, by Rokitanski, in Germany.

Of *transcendental anatomy*, Geoffroy St. Hilaire was the originator, some thirty years since. It is still receiving some attention, especially in Germany.

Progress of physiology and pathology since 1700.

II. The science of *function*—including physiology and pathology—has been advanced since 1700, mainly by the following individuals: 1. Frederic Hoffman, who became a professor in the university of Halle in 1700, and was the first to establish the true basis of these two departments.* 2. Boerhaave, who became a professor at Leyden, in 1710. He is the author of the humoral pathology, as now generally understood after half a century of disrepute from the ridicule of Dr. Cullen; and he proposed a theory of inflammation more consistent with the present state of physiology and pathology than most which have followed it. His aphorisms and institutes of medicine were translated into every European, and also into the Arabic, language. 3. Haller, a pupil of Boerhaave, has been styled the father of physiology. He

* His scientific and literary labors were immense. The mere titles of the folio volumes written by him, as detailed by Haller, in his "Bibliotheca Medica," extend to thirty-eight quarto pages.

first maintained, about 1740, that sensibility and irritability are inherent in the nervous and muscular tissues respectively; an opinion opposed for nearly a century afterwards, and now entertained by all physiologists. He wrote, between thirty-five and forty volumes, varying from octavos to folios, on anatomy, physiology, pathology, botany, surgery, and other departments of medical science.

Goethe also, though a poet, gave a new impulse to the science of function, by the announcement, in 1790, of his brilliant deduction, the principle of "developed and metamorphosed symmetry"—or the doctrine that all the different parts of plants are but modifications of the same structural form. 4. But John Hunter has done more for comparative anatomy, and human physiology as illustrated by the former, than any other individual; and holds the highest rank as an anatomist, physiologist and surgeon. His ideas of inflammation, announced about seventy years ago, have been implicitly adopted by most writers to the present time, though quite at variance with the present state of pathological science. 5. Sir Charles Bell discovered the motor and the sensory function of the anterior and posterior roots, respectively, of the spinal nerves in 1807, and published his discovery in pamphlet form in 1810. Blumenbach, Hufeland, Treviranus, Valentin, Prochaska, Müller, Burdach, the Wagners, and the brothers Weber, must also be mentioned here; and Adelon, Bérard, Foville, Flourens, and Bernard. And last, Dr. Marshall Hall, the discoverer, in 1832, of the reflex, or diastaltic, function of the spinal cord—

the greatest physiological discovery since that of Harvey.

In the department of general *pathology* alone, I only mention the labors of Williams, Chomel, and Henle; though the list might easily be made a long one.

Progress of hygiene and therapeutics since 1700.

III. The third department, or the science of *conservation*, includes two subdivisions, viz.: *hygiene*, public and private, and *therapeutics*. The number is so great of those who have advanced the science of therapeutics, that I cannot undertake to enumerate them. It would include every eminent practical writer—medical, obstetrical, and surgical—from Hoffman to the present time. I need only say, that the science of therapeutics, or the treatment of diseases, in the three departments of practice just mentioned, has kept pace with that of pathology, on which all true advancement in therapeutics must be based; and that the science of hygiene, or the preservation of health, has also advanced together with physiology.

And at length the question presents itself, what is the *present* state of our science? And I will attempt as brief a reply as possible.

PRESENT STATE OF MEDICAL SCIENCE.

I. We may regard descriptive and surgical anatomy as nearly complete in their development. Pathological anatomy, also, so far as its *facts* are concerned,

may be regarded as susceptible of no very great additional advancement, unless some new aids to observation are adopted. The 120,000 bodies inspected *post mortem* by Rokitanski alone, we may well suppose, presented almost every structural lesion cognizable by our present means of observation, that the human body is liable to. But deductions of great importance and value are yet to be made from this vast array of facts. Rokitanski, like most pathological anatomists, is merely the cautious observer and the accurate reporter. Some deductive mind will yet give these observations their true scientific value.

In *histology* many facts are yet wanting, and will be supplied. Here also the labor of a deductive intellect is required to coördinate the facts ; which promise future discoveries not inferior to those of Harvey and Hall.

Microscopic anatomy, aside from histology, which is a particular phase of it, needs, and will acquire far greater definiteness, and advancement. Of *comparative anatomy* also the same may be remarked, from its importance in elucidating human physiology and pathology.

Present state of physiology and pathology.

II. On the recent rapid advancement of the science of function, we have much reason for congratulation. But we must still affirm that the physiology and the pathology of the present day are altogether *too intensely chemical ;* and simply because we

implicitly receive our physiology, at the hands of chemists. Certainly the science of physiology cannot be formed without the aid of chemistry; but chemists should not, therefore, claim the whole field for themselves, nor should physiologists to such an extent have surrendered it. Chemistry conducts us to the threshold of physiology; but it can carry us no further. It is, therefore, a valuable and an indispensable guide thus far, but can be nothing more. But the chemists assuming, as did Paracelsus 300 years ago, that all the functions of the living body are merely the result of chemical actions,* virtually include physiology within their own domain. They next assume that a man weighs a certain number of pounds, say 140, and proceed to tell us how many ounces of food, both solid and fluid, he requires daily and per annum; just how much oxygen he consumes, and how much carbonic acid he exhales, and just what proportion of his weight he secretes in bile or gastric juice; and this they call making physiology a *positive science.* Physiologists, on the other hand, assert there is a distinct vital force; but still admit that possibly, after all, this force may be correlated with, or convertible into, chemical force, which is practically yielding the point to the chemists. Besides, we are reminded, that physiology is an *inductive* science, like chemistry; and therefore to be advanced in a similar way, and of course equally well, if not best of all, by chemists, from mere chemical facts.

* Liebig, however, asserts distinctly that the vital force is a force "sui generis."

But against this we enter our protest, for the following reasons :

1st. It is a principle to which there should be no exception, "never to attempt to solve the problems of one science, by the order of perceptions peculiar to another ;" since each science has its own canons of interpretation and investigation and reasoning.

2d. If Physiology *were* mainly an inductive science, like chemistry, I have shown that physiologists alone can judge of the value and the true bearing of physiological facts, and they alone can collate them for the inductive process (pp. 20–21).

The doctrine of final causes.

But while chemistry is merely an inductive science, in the sense already assigned to that term —it being far more inductive than deductive— *physiology* is something more than an inductive science. It is at the same time both inductive and *teleological.* In other words, it is partly inductive, and partly relies for its advancement on the assump- of final causes. Prof. Whewell asserts that the "assumption of final causes has given rise to the science of physiology ;" but if so, it consists, *also*, of principles obtained by the inductive process. The phrase "final cause" originated with Aristotle,* and is merely synonymous with the word "purpose." The doctrine of final causes has recently been expressed

* Aristotle's expression is, *τὸ οὗ ἕνεκεν—καὶ τὸ ἀγαθόν*, as indicating this kind of cause. See his Metaphysics.

by the word "teleology;" this word having reference, from its derivation, to the end, object, or purpose, of the part or organ under consideration. *E. g.* In order to ascertain the function or use of a part, we begin by assuming that it has *some purpose*—was formed for some *particular end;* and reasoning out that end or purpose from the structure and other circumstances, is applying the doctrine of final causes or reasoning *teleologically.* Bacon * regarded the "philosophy of final causes as sterile," and so does Geoffroy St. Hilaire. But it is the fact, that the use of every organ has been discovered by starting with the assumption that it must have *some* end.† The discovery of the circulation was due to the persuasion of a purpose in every part of the circulatory apparatus. In the science of function, we must, as Kant has remarked, "adopt the maxim, that nothing is *in vain;* and proceed upon it in the same way, in which in natural philosophy, we proceed upon the principle that nothing happens *by chance.*"

Practically, therefore, the physiologist must adopt the principle of final causes; a principle entirely unknown to chemistry, or any other merely physical science, and peculiar to the physico-vital sciences—or physiology and pathology. He may say, with Geoffroy St. Hilaire, "I take care not to ascribe to God any intention; for I mistrust the feeble powers of my reason. I observe facts merely, and go no further. I only pretend to the character of the his-

* "It is so far from being beneficial, that it even corrupts the sciences, except in the intercourse of man with man."—*Nov. Organum*, book ii., Aphorism ii.

† Appendix, note ii.

torian of what *is*." But after all, he cannot advance a step without the aid of the very principle he condemns.

All departments of science ask the question "why."* But while the physical sciences mean by this word, "through *what cause?*" the physico-vital mean by it, "for *what purpose?*" *E. g.* if the question be, "why do sulphuric acid and soda unite to form a salt?" the answer is a statement of the efficient cause, viz.: "because they have a chemical affinity for each other." But if the question be, "why have all animals a stomach?" the reply is, "in order to digest their food," and consists in a mere statement, not of the cause, but of the *object*, or *purpose* of that organ. While, therefore, chemistry is an *inductive*, physiology is a *teleological* science.

But physiology is also in many respects inductive. If we find that arsenic, administered in a certain dose to several animals of the same species, and in all respects alike as nearly as can be ascertained, proves invariably fatal—and deduce from these similar facts the general conclusion that arsenic is a poison to that whole species of animals, we are pursuing an inductive process. While, therefore, chemistry is merely an inductive science, physiology is both teleological and inductive; and the blending thus of efficient and final causes is peculiar to this subdivision of medical science.†

There is, indeed, a "tendency in all recent re-

* See note, page 27.

† That it is also deductive has already been shown by some of the discoveries adduced.

search," says a recent writer, "to impugn the doctrine of vitality, both in animal and vegetable life, as a distinct force and power; and to merge its alleged functions, whether of organization, maintenance, or reproduction, in those same physical forces, which act on the inorganic matter of the world around us." It is high time this tendency were checked; since physiology (as he continues,) "though closely encircled round by physical laws and phenomena, and approached only through these, has still a secret region within—the law and principle of life, hitherto inaccessible by any method of human inquiry"—though we are "moving onward to those truths, which are the certain reward of all legitimate inquiry.* "The notion of life and vital force," may still "be too obscure to be steadily held," as Prof. Whewell remarks; but it may gradually become so clear and definite, as to be available in science; and then, as Cuvier suggested, "natural history may have its Newton."

We, therefore, accept chemistry "as a means of *exploration*, not of *deduction*—as a pillar, not a pinnacle—as an instrument, not an aim. Chemical laws are *quantitative*, chemical actions resulting in definite combinations; physiological laws can never become *quantitative*, but only *qualitative*, since living substances or tissues have not a definite composition. No analysis of a nerve will ever throw any light on sensibility; no arrangement of chemical formulæ will ever explain the form and properties of a cell."

* Edinburgh Review, July, 1858.

Chemistry may tell us "what an organism is made of; but it cannot tell us how it is made, nor how it manifests its vital attributes."—*Blackwood's Magazine, March*, 1858.

Perhaps I can adduce no better instance of the barrenness of chemical labors as applied to the explanation of physiological phenomena than the *positive physiology*, so called, which chemists have conferred upon us, in respect to the subject of food and digestion; and which are familiar to most medical men.

During the last twenty years, the most extensive and laborious researches have been made by chemists in respect to food; and in a chemical point of view, merely, they are doubtless of very great value, for all this must precede the true physiology of food and digestion. But no important and reliable physiological result has yet been obtained from them. We are assured by the chemists that the nutritive properties of the various kinds of aliment are in proportion to the amount of nitrogen they contain; and that while the nitrogenized elements of our food are alone histogenetic, or *tissue-producing*, the non-nitrogenized elements are simply calorific, or *heat-producing*, and not in the least histogenetic. But physiological observation shows that the body may be nourished for a time, by the calorific elements of the chemists alone; and that the so-called histogenetic elements are also calorific. According to the chemists, fat being non-nitrogenized, should be calorific merely; but not a cell nor a fibre can be formed without a certain amount of fat, and it is, therefore, also histogenetic. Common salt, also, and the phos-

phates of lime and magnesia are as truly histogenetic as the nitrogenized compounds, albumen and caseine; for not a tissue nor a fluid in the human body can be formed if either of them is absent.

I will adduce only one more illustration of the positive physiology which is formed in the laboratory. In explanation of the fact, that all the different varieties of the human race are inclined to the use of some form of stimulant, the chemists have assumed, that the active principle of coffee, tea, and spirituous liquors, increases the functions of the nervous and muscular systems, without, or with greatly diminished, waste. I say, have *assumed* this, for no proof whatever of this position has been adduced. All admit that the vital phenomena in all *ordinary* circumstances are accompanied by waste of the tissue manifesting them. A muscle cannot produce the least motion without a corresponding loss of its substance; not an idea flits through the mind, without carrying away some molecules of the brain-tissue. But chemists assure us that if we first introduce a quantity of alcohol, or caffeine, or theine into the circulation, we may then move and think, *ad libitum*, without any sensible waste of the active tissues. A mere mechanic would doubtless explain this proposition, by saying that these three substances are oil to the corporeal machinery, and diminish the friction; and this explanation is as valid as the preceding, though it assumes, as did the iatro-mathematicians (p. 45) that the human organism is a mere machine. We use coffee, and tea, and alcohol, not to prevent waste, and, therefore, save the expense of food to

supply it; but simply because they are *stimulants*, and produce a certain well-known physiological effect; because they excite the nervous and muscular systems, especially, to a higher degree of activity for the time being; though the waste is here, as always, proportional to that activity. It is the waste consequent on action, that exhausts a tissue or organ; and if alcohol, coffee and tea, could enable us to think, and feel, and move, without waste of the cerebral substance and the muscular system, habitual drinkers would be the most intellectual of men; and their brain, never feeling the effects of exhaustion, would be superior to the necessity of sleep and repose. I am not here speaking in derogation of these substances properly used and administered; I am only objecting to the chemical explanation of their action, as totally at variance with all physiology, though it is unhesitatingly accepted by some physiologists.

Now, the errors before mentioned in regard to food, result from the fact that chemists merely look to the composition of the substances which nourish the organism; while the physiologist looks also, and more especially, to the *organism* which is *to be nourished;* and remembers that the latter *acts upon*, and changes, as truly as it is acted upon *by*, the food. Belladonna, hemlock, nux vomica, and aconite, are fatal poisons to most animals; but the rabbit takes the first, the goat the second, a kind of buzzard the third, and the horse, aconite, with impunity. Bread and milk, taken as food by a kitten, in time develop a full-grown cat; but taken by an infant, develop human tissues. It is especially the organism, then,

and not merely the chemical composition of the food, which determines and controls the result.

Positive physiology objected to.

We then accept the chemical ideas of food with much hesitation ; and especially when we also remember that, as Mulder and Lehmann have admitted, "chemistry is still in too imperfect a state to give clear and satisfactory answers to its own appropriate questions in this direction." The "positive physiology," I am considering, is, at most, nothing more than what Berzelius happily termed, the "physiology of probabilities." It is an attempt to erect the science of life on a basis of mere analysis, *i. e.* of death and decomposition ; and the result only too nearly resembles that achieved by the alchemist, as shown by the following epitaph, supposed to have been written by himself.

> "God his almighty power displayed,
> When he from *nothing all things* made;
> With crucible, to work I went,
> And in the fire a fortune spent;
> Reversing just what he had wrought,
> I *everything* to *nothing* brought.

I have confined my illustrations to the *physiology* of the present time ; but *pathology* is merely morbid physiology, or physiology modified by disease, and is at present equally amenable to the strictures I have applied to the department on which it is based. And it is high time that both physiologists and pathologists, while grateful for the aid which analyti-

cal chemistry has conferred, at length assume the control of their appropriate domains.

Present state of hygiene and therapeutics.

III. I cannot delay to do justice to the present advanced state of the science of conservation, *i. e.* of *hygiene* and *therapeutics.*

1. Public hygiene has of late received especial attention in all civilized countries ; and the results have been apparent, in the greatly diminished mortality in those cities and States where its principles have been best applied. We have, however, yet seen but the beginning of what can be accomplished in the saving of human life, and the prevention of disease ; and we never can fully realize it until the laws pertaining to the public health are administered and enforced by medical men. So long as health officers are selected on merely political grounds, and to the exclusion of medical men, just so long will perish multitudes annually of those who might have been saved by timely precautions. Nothing short of a thorough acquaintance with the various departments of medicine can render one familiar with the complicated relations of the great subject of the public health ; and not till every city inspector, coroner, and health warden, is a well educated medical man, can the people enjoy the sanitary advantages to which the present state of science entitles them.

Private hygiene, on the other hand, though well understood by medical men, can be to no great

extent enforced; since it is set at defiance by many of the predominant fashions of the day.

Nature in the cure of disease.

2. Of the present progressive and rapidly improving state of the science of *therapeutics* also, I must but briefly speak. We are approaching the period, and, I trust, are very near it, when it will be generally admitted that the first question proposed to himself by the physician on examining a patient and deciding the nature of the case, should be, not "*what* remedy shall I prescribe?" but "shall I in this case prescribe *any* medicine?" For if nature, aided by repose and appropriate diet, is sufficient in any case, promptly to accomplish the cure, surely all medication is unnecessary, and probably injurious. If nature is *not*, thus aided, sufficient, then occurs the second question, "what remedy shall be given, and what is the least amount that will accomplish the required object?" And he is the best practitioner who knows, both when to decide to give no medicine, and also what and how much to administer, when a remedy *is* required. To precisely this kind of discrimination the science of therapeutics is now tending. The result is, that far less medicine is administered now than twenty years ago, by the best practitioners, though with an increased, instead of a diminished faith in its effects. We sometimes hear it remarked that the older a practitioner becomes, the less is his faith in the efficacy of medicine. It would, I think, be more correct to say, the older a practitioner

becomes, the greater is his faith in the curative powers of nature. This will lead him to administer less medicine on the whole, for he will have learned to distinguish the cases requiring medical aid from those which do not. But his faith in medication, applied in cases requiring it, will be found actually to increase with age, and an increasing skill in applying it. As Richter has said, "He that is not growing wiser, has never been wise;" so I should say, that the practitioner whose faith in medication, properly applied, does not increase with his experience, has never had any faith based on his *own proper application of it.**

The science of therapeutics has also been much advanced of late by improved *forms* of therapeutical agents. The vegetable alkaloids, possessing a definite chemical composition and therapeutical power, have vastly increased the certainty and reliability of our prescriptions; and much more remains to be done in this direction. We therefore welcome any effort, from any and every source, to increase the certainty of the materia medica; and though we object for reasons before assigned (p. 13) to the term "positive," as applied to remedial agents, we consider that podophyllin, leptandrin, hydrastin, and several other remedies of this class, are already

* The work entitled, "Nature in Disease," by Sir John Forbes, who has had a medical experience of over half a century, has by many been entirely misunderstood. It merely inculcates a greater reliance upon nature in the management of disease, than has hitherto been manifested; while it asserts an undiminished faith in remedies, properly applied. It cannot fail to be productive of much benefit to the profession.

proved to be highly valuable contributions to our therapeutical means.

The numerical method.

And another recent effort to render therapeutics a positive science I must not entirely pretermit, since it still exerts a sensible, though much diminished influence. I allude to the "numerical method," initiated by Louis about twenty-five years ago. The aphorism "Ars tota est in observationibus" I have already called in question; and Morgagni added the remark about 200 years ago: "Non numerandæ sed perpendendæ sunt observationes." Louis, however, insisted that the enumeration of observations is the principal thing necessary; and that without this method, nothing valuable can be established in anatomy, pathology, or therapeutics. And Bouillaud, in his essay on medical philosophy,* travesties Morgagni as follows: "*Non solum* numerandæ, *sed etiam* perpendendæ, sunt observationes;" thus entirely reversing the meaning of the Italian medical philosopher. The science that does not count, say the numeralists, is never sure of anything, and therefore is not science; and, hitherto, medicine has not counted, or what amounts to the same thing, it has counted badly. It has said "very many, a few, several, often, rarely," and the like; it *should* say five, twenty, or some definite number, and five, twenty, or some other precise *per cent.*

No one fails to appreciate accurate counting in all

* Paris, 1836.

cases of statistical details; and so far as the numerical method merely counts, its value is not questioned. But when it also applies the inductive process, or the deductive, to its numerical results, and claims to be an infallible guide in medical experience, it simply attempts what, from the nature of the case, it can never fulfill. But it has promised its greatest triumph in its application to therapeutics; and I will specify some of its actual results in this department.

In the *first* place, the results of the practice adopted by the numeralists themselves, do not demonstrate a more reliable knowledge or an improved state of therapeutics on their part, since the mortality has not been less than that occurring in the practice of others. *E. g.* according to a report on typhoid fever, by Andral, of those treated by the expectant method, all recovered; while of those treated by purgatives and bleeding, by Bouillaud and Louis, one-eighth to one-sixth died.

But, *secondly*, the figures obtained by *different* partisans of the numerical method, entirely disagree; *e. g.* Louis, treating typhoid fever by free bleeding, lost one-half of his patients, and we should infer that it is a very dangerous practice; but Bouillaud, adopting the same treatment, lost only one in seventeen patients; from which we must infer precisely the reverse. Which of the two shall we follow, if our therapeutics is to be rendered *positive* in this way? Is blood-letting, in typhoid fever positively injurious, or, on the whole, beneficial?

Thirdly, the therapeutical result arrived at by

the numerical method are mere *averages*, and therefore of no practical value in particular cases. Bouillaud treated 57 cases of pneumonitis by successive bleedings and other remedies; though I here notice the results only so far as the amount of blood taken from the patients is concerned. On counting accurately, he found the 57 patients had lost 259 lbs. of blood, or on an average 4 lbs. 8½ oz. each. But not a single patient had lost precisely 4 lbs. 8½ oz., for a *patient is never an average*, but a case *by and of itself.* The greatest amount taken from a patient had been 10 lbs; and the least, 1 lb. 12 oz. Finally, of these 57 patients 53 recovered, and 4, or about $\frac{1}{14}$, died. Now, what is the therapeutical principle herefrom resulting? That if we treat pneumonitis by repeated blood-letting, we shall lose only 1 patient in 14? By no means, for much depends on the quantity of blood taken. Does it then follow that if we take 4 lbs. 8½ oz. of blood in cases of pneumonitis, we shall cure 53 out of 57 cases? Certainly not, for that treatment was not adopted in the cases adduced, and would probably have led to a different result if it had been. It, then, neither proves that blood-letting *is* or *is not* a proper remedy in pneumonitis, nor how much blood should be taken, if bleeding be decided upon. From the cases adduced, we can only infer that if 57 other cases of pneumonitis, individually like the 57 before mentioned, should be again individually treated in the same way—the amount of blood taken varying from 1 lb. 12 oz. to 10 lbs., and averaging 4 lbs. 8½ oz.—53 of the 57 cases would recover. But we know we can never again collect

57 cases precisely like the preceding; and, therefore, though this conclusion may be very *positively* asserted, it is *positively* worthless, except as a mere statistical fact.

The numerical method can therefore never add essentially of itself to the department of therapeutics. It can count, and count well; and should pretend to nothing more. But when it oversteps its appropriate domain of mere statistics, and essays to deduce conclusions, it ceases to be a reliable guide. Its facts, as obtained, are too heterogeneous to warrant an inductive conclusion, and can only be conducive to science in this way when obtained under precisely similar conditions—the most difficult of all things for the observer in this department to be sure of—and when collated by an adept in pathology and therapeutics. It may be also added, as a matter of fact, that not a single important discovery in therapeutics has by this method been made, while meantime the anæsthetic effects of ether have been discovered by a mere deduction and its verification; and subsequently, those of chloroform and amyline, by deduction also from the known similarity of action in other respects of chloric and sulphuric ether, and the highly carbonized composition of them all. Of course all knowledge of therapeutics must proceed at first from direct experiment; but as science advances, deduction will become more and more useful in the way of discoveries in this department; our deductions being verified by experiment, as in the instances just given. For as our methods of diagnosis become more certain, we shall be able, more and more, to

deduce the therapeutics required in each particular case from its pathology and our knowledge of the action of remedies. Sydenham, who was contemporary with Bacon, Harvey, and John Locke, recognized this principle in our science over two hundred years ago. "As truly," says he, "as the physician may collect facts of diagnosis from the minutest circumstances of disease, so truly may he elicit indications in the way of therapeutics. I have often thought that, provided with a thorough insight into the history of any disease whatever, I could invariably apply an equivalent remedy. These phenomena of the disease, if carefully collated with each other, lead, as it were by the hand, to those palpable indications of treatment, which are drawn, not from the hallucinations of our fancy, but from the innermost penetralia of our nature."

But I have stated that therapeutics is mainly an inductive department of our science. Facts alone, therefore, well assorted and in sufficient number, warrant, in most instances, a practical deduction; and hence the most rigid observation and experiment are here indispensable. But it is in this department especially that we realize the force of Hippocrates' maxim, "Experientia fallax—judicium difficile;" since the action of therapeutical agents is constantly modified by the ever-varying conditions of the living organism. Besides, the testimony of the observer's senses are quite liable to be biased by some preconceived opinion, if not compromised by some defect in his method. Hence it is generally far safer to wait till a second observer has verified the so com-

mon announcements of wonderful properties of new remedies, or new applications of old ones, before we adopt them in our practice. Too generally have the novelties in this department been found to depend on some dominant hypothesis or theory; as the elephant seen by the too credulous astronomer in the moon, turned out to be a mouse in the distal portion of his telescope.

The spirit of medical science.

The facts and ideas which have been presented, show that we may very safely abate some of the pretensions often put forward, in regard to the present perfection of our science; though we cannot still say with Bacon, that it is "a light and slender thing." On the other hand, we have cause for admiration and astonishment at the vast and imperishable structure which has been erected during the last one hundred and fifty years; and may confidently anticipate at least an equally rapid advance in future. We have found its progress thus far, interrupted by long stationary periods, and that it has at times even retrograded. In the future we can hardly expect such interruptions. We have seen that in its *spirit* our science was mystical to the time of Hippocrates; exclusive thence to Galen; servile and acquiescent from Galen's time to Harvey; and uniformly progressive since Harvey's discovery became generally accepted, to the present time. That the present philosophical and progressive spirit will still continue to advance and ennoble our science, we cannot entertain a doubt.

PART III.

THE CLAIMS OF MEDICAL SCIENCE UPON MEDICAL MEN.

And, finally, the question occurs, what are the claims of our science upon us as medical men?

This question would be answered very differently by different members of our profession, since they cherish very different kinds of attachment to science, as is expressed by a German poet:

> "To some she is a goddess great;
> To some a milch cow of the field:
> Their business still to calculate
> *The butter she will yield.*"

But our science has *claims* upon us all alike; and I may indicate the manner in which we should respond to them, in the words of the author of the "Novum Organum:" "I hold every man a debtor to his profession; from the which, as men do seek and receive countenance and profit, so ought they of duty to endeavor themselves to be a help and an ornament thereto." It is often repeated that ours is a noble science; then we can become an "ornament thereto" only so far as we are individually scientific and noble men. But we may be a "help" to our science in two ways; 1, by our own labors, and 2 by encouraging the labors of others.

How improve our science by our own labors.

I. There are three principal methods of improving our science by our own labors: 1, by ascertaining and correcting existing errors; 2, by collecting and arranging established facts in a scientific form, in monographs, treatises, etc.; and 3, by making new additions to it, or actual discoveries.

1. In order to ascertain and to correct existing errors, it is necessary to question and reëxamine all ancient opinions. We have seen that men often continue "acquiescent in errors so baseless, that they vanish immediately they are challenged." We must then challenge old opinions. I do not inculcate skepticism; that is usually the offspring of ignorance. I bespeak that rational and temperate doubt, which an acquaintance with the history and development of all science cannot fail to cherish. "He who seeks for truth," says Descartes, "must, once in his life, doubt of all he believes." I would encourage the habit of scrutinizing the doctrines and opinions of others; and, above all, of forming our *own* opinions, upon data which we possess in common with them; in a word, of being "*Nullius* addictus jurare in verba magistri." The habit of thinking for ourselves may seem to imply that of observing for ourselves; for we have seen that in the formation of science, sense and reason—observation and deduction—have a mutual need of each other. But we are to remember the difficulty of observing correctly; and that while "the knowledge of the senses is the best of know-

ledge, delusions of the senses are the worst of delusions."* Each, therefore, should ask himself if he has the qualifications for a good observer, and the leisure for making observations; and, if not, he may rely on others for facts, and restrict himself to the interpretation of them, as we have seen was the case with Harvey and Newton. In respect to independence of opinions, it will, however, always be true of the vast majority of even educated minds, as in the time of the author of the "Praise of Knowledge." "They learn nothing at the universities but to *believe*. They are like a burdened ship, they never move, but by the wind of other men's breath, and have no oars of their own to steer withal!" We wonder at the poverty and servility of the human mind in the middle ages. But the number is comparatively very small in every age, of those who, thinking and reasoning independently, advance their science, and compel the rest to follow, though with lagging and unequal steps. Without the independent thought of Vesalius, Cæsalpinus, and Harvey, the circulation of the blood might have remained undiscovered to the present time. There are now many important subjects in the various departments of our science which require to be investigated entirely *de novo*. Digestion is one of them, as we have seen. And none should be deterred by their obscurity, from the expression of their well-formed opinions. Every man is obscure in science till he makes himself otherwise; and young men have ever done more

* Dr. Latham.

for science than those far advanced. Vesalius published his great work on anatomy when twenty-nine years of age; Andral published his on Pathological Anatomy when but twenty-seven years of age; and Bichat had completed all his immense labor at the early age of thirty years.

2. After reëxamination of the received facts and principles of our science, and the rejection of such as are found to be false or untenable, we may contribute much to its future advancement by others, or by ourselves, by collecting the established facts and principles, and arranging them in a systematic order in the form of a compendium, a monograph, or a treatise on the particular subject or department concerned. Thus the labor of acquiring that department, up to the present time, is vastly abridged; and the student, and even the practitioner who has but little leisure, can gain that amount of knowledge which is indispensable, in no other way. The compendium, or treatise, however, affords only a panoramic view, as it were, of the particular department of science; the details must be subsequently sought in the monographs on the particular topics it includes.

3. To those who would advance our science by actual discoveries, whether of facts or principles, the following considerations are of importance.

First. Those who would become discoverers, should evidently commence their preparation by accurately acquiring all that is already known in the department they propose to advance. Many an individual who prides himself upon his talents for original investigation, has found, after arriving with much labor at

a particular result, that another had already done, and perhaps much better, precisely what he had accomplished. A vast amount of intellectual power is absolutely thrown away, from this kind of ignorance. When we have acquired all that others have achieved, or have learned where, why, and in what way, others have failed, then, and only then, may we exert our own powers with the best prospect of success.

Secondly. If we would make additions to our science, we should have a definite idea of the precise object we wish to accomplish. Thus alone can we propose a definite question to the mind; which is the only way to achieve a definite result. For "rightly to state a problem, is no inconsiderable step towards its solution."

Discoveries are of two distinct classes.

Thirdly. The future discoverer in our science must remember at the outset, that in respect to the methods for achieving them, discoveries may be arranged in two classes: the *observational* and the *rational*. The first are discoveries of facts alone, and are made by the use of the senses merely—by *observation;* the second are discoveries of principles or laws, and are made by the use of the reason, by *deduction*, and are afterwards verified by facts. In the first, the discoverer merely *sees;* in the second, he *foresees*. The discovery of a continent, an island, a comet, a new chemical element or compound, of a muscle, or of any anatomical or histological element, are instances of the first class of discoveries; those of

Harvey, Kepler, Newton, Dalton, and Hall, belong to the second class. The former may sometimes be made even accidentally, or by one of inferior intellectual capacity, if favorably situated ; the latter demand a philosophical and logical mind of a high order, with habits of intense and continuous thought. This continent was actually discovered by Columbus because it lay in his course while he was intending to go to Asia, and really supposed he was doing so. Besides, the discovery was really made by the sailor at the mast-head of Columbus' ship ; but then its commander had placed him there to keep a look-out for land. Not all facts can, however, be discovered by the senses alone ; and when the reason is also required to ascertain them, they belong to the second class. Such is the fact that the earth moves round the sun, and that its orbit is elliptical, and not circular. I do no injustice, therefore, in adding that the authors of the first class of discoveries—those by mere observation—are mere *finders ;* while the others are the true *discoverers.* Both are equally seekers after truth, except in the instances in which facts are accidentally ascertained ; and both must institute definite investigations in order to accomplish their object. But those who seek for facts alone have ever made the greatest parade respecting their investigations ; and this too, generally, whether they lead to any positive result or not. So that now-a-days it seems to be by some accounted a greater glory to be said to be engaged in original investigations, though they never report a new fact discovered, than to have actually discovered a new law in science.

And here I will devote a few lines of criticism to this phrase, "original investigations," so often used in a somewhat towering sense at the present time, by those who profess to be engaged in them. *Investigation* means "the action, or process, of searching minutely for truth, facts, or principles" (Webster). Of course, its methods are various, but they all imply the use of the senses alone, as in the search for facts, and sometimes for truths; or of the reason more especially; or of both combined. The most facile method is that which exercises the senses alone, and this everybody can profess at least, though very few will achieve discoveries even of facts. On the other hand, but few can exert the combined influences of the senses and the understanding in their investigations. The other party will, therefore, from their number, raise the greatest clamor respecting their own labors; while, at the same time, the mind of larger grasp and rational tendencies, usually waits till it can modestly state what it *has done*, instead of proclaiming what it is *doing*. Besides, the mere seekers after facts, can display to the uninitiated their instruments of brass and of glass, packed in rosewood cases, or arrayed upon the walls of a laboratory; while the rational discoverer carries the main part of his apparatus for discovery entirely out of sight, within his cranium. But the word "original" has, also, to some, a sort of talismanic and exclusive signification, as applied to their own scientific labors. Indeed, some gentlemen seem to attach far more importance to the originality, than to the investigations themselves, or their results. To be engaged in

"*original* investigations," is, forsooth, the *plenum* of their scientific aspirations; though their labors prove as barren as the observations of the eastern fakir, who spends his days in the contemplation of his omphalos. *Original* means "first in order; preceding all others." But whether mere priority confers any special merit upon our investigations, depends upon their objects, their methods, and, above all, upon their results. The greenhorn who should attempt to ascertain the dimensions of the man in the moon through a telescope of his own construction, would, so far as is recorded, be engaged in an *original* investigation. But, should any especial renown accrue therefrom? The alchemists made many original investigations in search of the philosopher's stone, and the universal solvent; and they also sometimes boasted of their labors. But all others, at the time, and even since, have derided them as visionary, ridiculous, and without results. He who first institutes a legitimate scientific investigation, deserves all commendation and encouragement, that he may attain to some important result; and when the latter is achieved, he may expect the praise of all scientific men; but it is sufficiently out of character for the true scholar to boast of a thing, even when it is *already done.* The less, therefore, we think of the originality, and the more of the objects of our investigations, and their results, the better is the prospect of advancing our science. The mere seekers for facts have, however, no exclusive claim to originality. To say that the investigations of Harvey were not as original as those of Fabricius, is simply absurd.

Originality falsely so called.

There are, however, two very cheap methods of acquiring a reputation for originality to which I may here allude. The first consists in the repetition, under some modification of circumstances, of the observations of others, and tacitly allowing or actually aiding them to pass for "original investigations." The other is adopted generally by those who boast of writing "original works" in the various departments of our science. Of course, two persons may make the same investigations, neither being aware that another had ever made them; and in such a case, both are entitled to the merit of originality, if that is of any account, though he only who preceded in point of time will retain it. But I am now alluding to what is at best but a mere imitation, and often a very lame one, of the experiments of others. On the other hand, upon most of the departments of our science, it is folly to pretend, at the present day, to write an original work. To assume that the facts and principles contained in such a work have all been obtained first, or originally, by the author, is assuming that they were before unknown; and therefore asserting that the work is one upon an entirely new department of science. An original work, therefore, at the present day, on descriptive anatomy, physiology, pathology, or either of the practical departments of medicine, is an impossibility. A *new* work on either of these departments may contain new facts or principles acquired by the author's original investiga-

tions, perhaps; or it may possess originality of plan or object, or arrangement, or in the manner of presenting the various facts and principles to the reader's mind. Indeed, if it is not original—"the first in order, preceding all others"—in some of these respects, it should never see the light, and will be very certain not long to enjoy it. Yet such a work is not, as a whole and unqualifiedly, an original work; or, if it be still asserted to be such, then a treatise or a compendium may be as deserving of this distinction as a monograph or an essay. A truly original scientific work is one in which original investigations, ideas, or principles—or all these together—predominate. Tested thus, it is true that original works are very rare. There is much writing with comparatively little new matter. Most that is done is "vertiginous, or in the way of perpetual rotation;"* and most authors who profess the most originality, fill their works with egotism instead. But original works, as just defined, are by no means indispensable to the advancement of science; while *new* works are so, in order to incorporate into it, in a permanent form, the new facts and truths which are successively ascertained. On the other hand, also, an original work, like original investigations, as has been shown, may be entirely barren and useless. The question, then, suggested by the announcement of each new scientific work should be not, "is it original?" but "is it truly useful?" *i. e.* will it conduce more than its predecessors to the advancement of science?

* De Augmentis, p. 1.

Moral qualities required in a discoverer.

Fourthly. Certain moral elements are also demanded in the future discoverer of scientific truth. I first mention *faith*, as opposed to skepticism and infidelity. He must constantly remember that

> "There is more in heaven and earth
> Than is dreamt of in *his* philosophy,"

or that of his times. And a higher faith is a very essential aid; and hence, as Prof. Whewell has remarked, "discoverers in science have generally had a belief in an intelligent Creator."*

Fifthly. Another important moral element is a patient *perseverance.* Habits of intense and continuous thought are indispensable to the real or rational discoveries, and hence they are not rapidly or easily made. When Newton was asked how he made his discoveries, he replied, "by constantly thinking of them." He also thought and labored for seventeen years after his discovery of the law of gravity, before it could be said to be demonstrated and completed. Harvey's work cost him a labor of twenty-six years; and Marshall Hall states that he devoted to his discovery of the reflex or diastaltic function of the spinal cord, not less than 25,000 hours of intense thought, and as many more to its applications to diagnosis and pathology. Hence it is that the greatest discoverers have generally been men of the highest intellectual endowments, and of extensive general, as

* Vol. iii. p. 477.

well as professional, acquirements. Harvey, Newton, Haller, Jenner, and Sir Chas. Bell are examples.

Finally. *Courage* and *independence* are invaluable aids. He who would hope to make any real discovery in our science must, at all events, have no dread of being called a theorist. We have seen that a *rational* discovery is necessarily preceded by a theory, and which, on being verified, becomes the discovery itself. Newton's discovery of the law of gravity was a mere *theory*, based on Kepler's law, till it was verified by facts applied by Newton's intellect; Kepler's discoveries were also, till verified by him, mere theories, based on Copernicus's discovery, that the sun is the centre of the solar system; and Copernicus's discovery, too, was in like manner at first a theory. It is, of course, only rarely that a theory leads to a discovery; but it is always true that a rational discovery is first shadowed forth in the discoverer's mind as a mere theory, and which he subsequently tests, and verifies if true. The greatest philosophers and discoverers have therefore always been theorizers; and it would be interesting to know how many diverse theories had been adopted for the time, and afterwards rejected, by Harvey and by Newton, before the true theory, as verified in the discovery, presented itself to their minds. Kepler's account of his own experience in this respect, constitutes one of the most interesting chapters in the history of science. It has very rarely been the case that the true theory is the first suggested to the discoverer's mind; or if so, false theories have almost invariably been previously suggested by others.

Hence, almost all discoveries are preceded by, and associated with, less clear and definite speculations, as Newton's was by Kepler's fiction of magnetic virtue; and sometimes, when the discovery is at length actually made, it may be very doubtful who is really entitled to the merit of being the actual discoverer—the preceding theories having approached so nearly to, or having actually suggested, the true one. That he who proves or verifies the theory is the *real discoverer* is, however, the only rule to decide such cases. Hence it is very common to remark that others were on the way to the discovery, though they did not make it. This was the case with Dalton's law of definite proportions. Hence, also, two or more persons, at a distance from each other, may actually make the same discovery, neither being aware of the labors of the other. "Il est tout simple," says Dutrochet, "que l'on se rencontre, dans le chemin de la science." Hence, also, we may be on the threshold of a discovery, without being aware of it. Let no one, therefore, fear to be called a theorizer. His theory may suggest a discovery to another, if not to himself. Only let him know that he *is* theorizing, and not mistake his theories for actual discoveries—which they can never be till verified.

In theorizing we reason from ideas, not facts; or, in other words, we adopt the deductive, and not the inductive process. I have already shown that some of the greatest discoveries—the real discoveries as opposed to mere findings—even in the physical sciences, and more still in our own science, have been the result far more of deduction than of induction. And

this will probably be true to a still greater extent, we have seen, as science advances in the future. I have, therefore, insisted on deduction, as a not less important method, henceforth, of progress in our science ; though induction is by no means to be laid aside. We are evidently now in an age of transition. "Changes affecting the whole condition of mankind are occurring more rapidly, as well as more extensively, than at any other prior time in human history ;" the grand agent in this change being the wonderful progress, more especially, of physical science, and particularly of mechanics and electricity, and its modifications. "Inductions of a higher grade have been reached, and generalizations attained, going far beyond those subordinate laws in which science was formerly satisfied to rest."* And all this has been accomplished mostly by deduction. Our own science is also responding to this progressive impulse of the physical sciences ; and the more it advances, the greater will be the need of the transcendental views furnished by the deductive method. Great has been the progress of the last 150 years, but what is already known is "but a speck compared with what will be added." Medical science will be *complete* only when "the structure and the functions of all the tissues and organs in the body shall be understood, and all the counteracting agents to all abnormal structures and diseased functions, are ascertained ;" and the still unfulfilled objects of science will be achieved, we believe, more by the rea-

* Edinburgh Review, *ut sup.*

son than the senses—more by deduction than by induction. "In that field which we and our posterity have yet to traverse," says a profound writer, "I firmly believe that the imagination will effect quite as much as the understanding;" this faculty suggesting the ideas on which the understanding may reason. And "we have every reason to believe, that when the human mind once steadily combines the whole of its forces, it will be more than a match for the difficulties presented by the external world."

But the preceding qualifications for making scientific discoveries imply labor, and of course *time*. Can a physician command the time required? That will depend upon his habits. If he fully appreciates the value of minutes, he may do so; otherwise not.

> "Sands make the mountain—moments make the year."

I have often thought that the practising physician, to accomplish his duty to his profession, must realize in his own busy life, the stanza in Faust:

> "Geburt und Grab,
> Ein ewiges Meer,
> Ein wechselnd Weben,
> Ein glühend Leben."*

To accomplish much in aid of science, we must possess some of the industry of John Hunter, who, for thirty years in succession, rose before daylight to pursue his favorite labors. We must even somewhat

* "Birth and the grave—
An eternal ocean,
A changing motion—
A glowing life."

resemble the German philosopher, who had labored sixteen hours daily, for many years, except on the day of his marriage, when he lost two hours ; but who rose two hours earlier than usual the next morning, and thus regained the time he had lost.

II. The ways in which we may directly aid and encourage the efforts of others, to advance our science, are numerous, and need not be specified.

The Hinderers of our Science.

But there is always a class of men in our profession, who not only refuse to aid, but who consistently oppose, every effort at improvement, as mere impertinence, if made by a young man, or as mere folly or intentional humbug, if by one more advanced in life. Yet most discoveries have been made by men still young. Newton was but twenty-three, Copernicus thirty-three, Sir Charles Bell thirty-three, Fabricius thirty-seven, Kepler thirty-eight, and Harvey and M. Hall forty-one, when their respective discoveries were made. Never making an observation nor a deduction themselves, these persons seem to assume that nobody else has the right or the capacity to do either, and receive what is suggested in good faith, and after much labor and anxious thought, with suspicion and unkindness. I am bound to say that the number of such in our profession is very small ; though their opposition to all true progress (which they call innovation) is sometimes so unscrupulous and energetic as to counterbalance, for a time, their fewness in numbers, and much to discourage the

actual discoverer. These gentlemen, it is, who in their successive generations, have uniformly taken the same position in respect to all proposed discoveries in our science; asserting first, that it is no discovery at all; secondly, that somebody made it long ago; and thirdly, either that it is of no account, or is fraught with only mischief to mankind. They adopt the policy of the dog in the manger; and like him also, in respect to all scientific improvement, starve themselves.

But, on the other hand, discoverers themselves do not always remember that they must, from the very nature of their position *as discoverers*, be in advance of their age, and that, therefore, some time is required to elevate their contemporaries to their own point of view. Discoverers, therefore, sometimes lose their temper, or in their enthusiasm perhaps seem overbearing to others; who cannot possibly see precisely and as clearly as they do, because they have not traversed the same ground with them. A discoverer must expect, as a matter of course, to meet for a time with comparative indifference, if not actual opposition, for the reasons just mentioned; and few, therefore, have lived to see their discoveries generally accepted, however important the latter may have been.

Thus in our scientific relations even, there is need of mutual forbearance; but the true scholar welcomes every attempt at improvement, from whatever source; remembering that by encouraging the spirit of investigation and progress, he indirectly contributes to that consummation which is the scholar's

highest aspiration—that perfect state of our science which has been defined. Nor is he discouraged by failures; for with Goodwin he believes, that the "human man can conceive nothing which it cannot finally execute."

Fellows of the New York Academy of Medicine:

To advance our science and art, is the main object of this association; though it would at the same time, also maintain the dignity and honor of our profession. We have here no superstitious rites, or cabalistic mysteries, for we have a science *now;* we have no plans for self-aggrandizement; we only labor to make ourselves and our profession wiser, and better qualified to relieve the sufferings of our fellow-beings. But in the fulfillment of its objects, no external influences, no considerations which mix themselves up with the diverse motives and the opposing interests of daily life, must be allowed to obtrude themselves here; and if it become necessary to exert its disciplinary powers, it is better to err on the side of clemency than of undue severity. Science knows no party, clique, or school; but then she is dignified, liberal, charitable, magnanimous, and not narrow-minded and vindictive. She cordially extends the hand of fellowship to all ranks and conditions alike; conducting the poor medical student, previously a laboring blacksmith, to the professorial chair of the erudite Velpeau; and out of four penniless boys, the poorest of the poor, moulding a Galileo, a Kepler, a Davy, and a Farraday.

But when dissension enters, science takes her leave; and would that every member of my profession realized the ennobling influence upon himself of the spirit of kindness and courtesy towards his professional brethren. Like mercy—

> "Its quality is not strained,
> It falleth like the gentle rain from Heaven
> Upon the place below. It is twice bless'd—
> It blesses him that gives and him that takes."

Let us then remember we are *men*—we are *brothers*. Much has this Academy already done to advance our science, much in diffusing a spirit of kindness and liberality among its fellows. It now only waits for a local habitation, for an *edifice* commensurate in beauty and capacity with the taste, the liberality, and the wealth of this great city, to enable it to extend its benefits to our profession throughout every part of our land.

APPENDIX.

NOTE I.—*Page* 40.

Of the different branches of medical science, *anatomy* first took a systematized form. Although the Greeks transplanted the knowledge of Egypt to their own soil, Homer nowhere distinguishes the muscles from other parts, nor does Hippocrates even. The latter also includes under the same term (νευρον or τονος), the nerves, the tendons, and the ligaments, and also says these νευρα contract the limbs. He did not discriminate between the veins and arteries, calling them both φλεβες. Aristotle says, nearly one hundred and fifty years afterwards, that the νευρα originate from the heart, connect the bones, and surround the joints. He therefore means the ligaments, and not the nerves, as some have supposed. He also traced the veins to the heart. Herophilus, of Alexandria (B.C. 300), taught that the nerves are the channels of perception; but he also included the tendons under this term. There was now a stationary period till after the victories of Lucullus and Pompeius in Greece and Asia Minor, and the rush of learned men from these countries to Rome, to exchange their art and science for Roman wealth.

Galen, who was born about 500 years after Aristotle, speaks of the caste of dissectors, who began when children, and who neither needed nor possessed any books on anatomy, though books were needed in his time. The muscles and the structure of the bones had ere now been recognized, but nothing definite was known of the nerves; and Galen thought every muscle consists of a bundle of nerves and sinews. Yet he showed the necessity of the nerves, and the origin of the voluntary motor apparatus from the brain, which is also the centre of the soul; against Gryppus, who maintained that the heart is the seat of sensation and emotion. Galen, however, still believed that the heart is the seat of courage and anger, and the liver the seat of love. He held that the veins originate in the liver, and the arteries in the heart; and that there is an opening through the septum between the right and the left ventricle, and which was believed for 1340 years afterwards. He left a treatise on the Anatomy of the Nerves; but he dissected only seven pairs of encephalic nerves, beginning with the optic, which he called the first pair. He spoke of his anatomical work "as a religious hymn in honor of the Creator."

Galen died in the year 203, and the dark ages, so called, now ensuing, no additions were made to anatomy for the long space of one thousand years, during which time the authority in anatomy of Galen was supreme.

Mondini, Professor at Bologna, (see page 42), merely revived anatomy, without essentially advancing it. Like all the rest, for the preceding thousand

years, he also swore by Galen ; and Galen's work, with some descriptions of Mondini, was the textbook for more than two hundred years. Vesalius found Galen's division of the encephalic nerves into seven pairs to be incomplete. He also accused Galen of other mistakes, and of having dissected the lower animals only. He should be regarded as the founder of modern human anatomy. The figures of his work, "De humanis Corporis fabricâ," were designed by Titian. Eustachius, and Fallopius, are also grouped with him by Cuvier.

Not till about 1660 were nine encephalic nerves recognized, by Willis, who added the olfactory and the spinal accessory to the seven pairs of Galen ; and though the French nomenclature has long since admitted twelve pairs, all English writers still persist, on the obsolete authority of their countryman, in including the twelve within the limits assigned by Willis. The latter also described the corpora pyramidalia, Santorini having noticed their decussation, and carefully examined the various ganglia. The distinction, however, between the ganglionic system of nerves and the cerebro-spinal, was first made by Bichat about the year 1800.

Inasmuch as human dissections were not legalized among any of the ancient nations, the internal structure of the lower animals alone could be studied, and that of man be inferred by analogy. Indeed, Herophilus is said to have been the first by whom human bodies were dissected (about B.C. 300) ; permission having been given him by Ptolemy to open and inspect the bodies of living criminals, for anatomical and physiological purposes.

We must, however, by no means conclude that human bodies were not at all dissected, because human dissections were forbidden in ancient times. Medical men, doubtless, sometimes thought then as did Abernethy, that "if the dead are not mangled, the living will be;" and qualified themselves for surgical operations, independently of legal sanction, as was not uncommon in this country thirty or forty years ago.

NOTE II.—PHYSIOLOGY (P. 65).

Some notions of function of the various parts and organs of the body must have been coeval with the first observations of the organs themselves. We may therefore say of physiology, as of the art of medicine, that in the form of proverbs, maxims, aphorisms, or crude notions of more or less of the healthy functions, it has always existed among all nations. No attempt was however made to reduce physiology to a scientific form till very recently. Some suppose it a very easy thing to ascertain the function of a part, after examining the structure; but, with few exceptions, nothing is farther from the truth. Physiology, therefore, does not flow by mere inference from anatomy; and therefore discoveries in physiology have always followed those in anatomy, "longo intervallo." The ancient anatomists knew of the arteries and the veins; but Hippocrates did not know the function of either, and called them both by the same name. Afterwards it was held that the arteries hold air only, and the veins blood. Herophilus (300 B.C.)

taught that the nerves are the channels of perception; but then he included the ligaments also under the term nerve, and therefore could not have known their structure. Fabricius discovered the valves of the veins; but Harvey discovered their function 45 years afterwards. The anterior and the posterior roots of the spinal nerves were known to Galen; but their functions were not ascertained till about fifty years ago (1807) by Sir Charles Bell. The structure of the various parts of the brain has been laboriously studied; but who yet knows the functions of each of these particular portions?

And this follows from the very nature of physiological science, as compared with anatomy. Descriptive anatomy is essentially a science of observation; physiology is a science of observation and deduction, as has been shown (p. 66).